BIBLIOTHECA BODLEIANA

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TRACTS

Written

By the Honourable

Robert Boyle,

CONTAINING

New EXPERIMENTS, touching the Relation betwixt *Flame* and *Air*. And about EXPLOSIONS.

An HYDROSTATICAL Discourse occasion'd by some Objections of Dr. *Henry More* against some Explications of New Experiments made by the Author of these Tracts: To which is annex't, An *Hydrostatical Letter*, dilucidating an Experiment about a Way of *Weighing Water in Water*.

New Experiments,
- Of the *Positive* or *Relative Levity* of Bodies under Water.
- Of the *Air*'s *Spring* on Bodies under Water.
- About the *Differing Pressure* of Heavy *Solids* and *Fluids*.

LONDON,
Printed for *Richard Davis*, Book-seller in *Oxon*.
M DC LXXII.

Advertiſement to the Book-binder.

SOme of theſe Tracts having been miſplaced in the printing, the Book-binder is deſired to take care of placing the ſeveral Tracts in the order, as they ſtand in the *Title-page*; as alſo to obſerve, in the binding, the Advertiſement given *p.* 131. immediately following after the *Experiments about the Relation betwixt Air and the Flamma vitalis of Animals.*

NEW
Experiments,

Touching the Relation betwixt

Flame & Air:

And particularly betwixt AIR, and the *Flamma Vitalis* of *Animals*.

To which are annexed

Two Attempts; the one, to produce *Living Creatures* in *Vacuo Boyliano*; the other made upon *Gnats* in the same *Vacuum*.

THE PUBLISHER TO THE READER.

IT will, 'tis presum'd, be altogether needless to preface any thing by way of commendation to the following Tracts; they will certainly commend themselves by their own worth to the Intelligent and Attentive Reader, who might have seen them sooner if the Press had not detain'd them longer than was expected; since, to the Publisher's knowledge, they were ready in the Year 1671. *except the* Hydrostatical Discourse

and

and the Explication of the Author's Experiment of Weighing Water in Water, *the former of which was finish'd in the beginning of this Year* 1672; *though the latter could not be so till near the end of the same Year*, viz. *the month of* February English *stile, because the Book of Mr.* George Sinclair's *Hydrostaticks, in which it is excepted against, came not, I think, before that time to* London, *I am sure not to the view of the Honourable Author. Farewell.*

NEW EXPERIMENTS

Touching the Relation betwixt

Flame and Air,

Sent in a Letter

To the Learned Publiſher of the

Philoſophical Tranſactions.

SIR,

YOu may have obſerv'd as well as I, that ſince the publiſhing of the Experiments I ſent you, touching *Reſpiration*, divers of our Learned men have ſpent both Thoughts and Diſcourſes in inquiring and diſputing, Whether there reſide in the heart of Animals, ſuch a fine and kindled, but mild, Subſtance, as they

call a *Vital Flame*, to whoſe preſervation, as to that of other flames, the *Air*, (eſpecially as 'tis taken in, and expell'd again by Reſpiration) is neceſſary. This among other conſiderations makes me think it ſeaſonable (though many avocations make it inconvenient) to compleat the performance of the Promiſe I made you, by adding to the Experiments about Reſpiration, which your commands have already obtain'd of me, thoſe ſcatter'd *Notes*, that I have been able to pick up about the *Relation* betwixt *Flame* and *Air*: And though, I confeſs, they are very much inferior in number to the Tryals about Reſpiration; and that in making them it was not ſo much my Deſign to compleat an intire and diſtinct Tract, though but a ſmall one, of ſuch Experiments, as to gratifie my own curioſity in the examining of a Paradox or two I had

had been writing about Flame; yet the nobleneſs of the Queſtion now under debate, and their pertinency to it, will poſſibly keep them, as few as they are, from being uſeleſs. And that alſo they may be the better kept from being unwelcome, I have choſen to make my ſelf a Relater of matters of fact, without ingaging with either of the Litigant parties in a Controverſie, wherein I am the leſs tempted to be partial, becauſe I have not formerly declared my opinion about it, and at preſent, I ſee, on *either* ſide, Perſons for whom I have no ſmall reſpect and kindneſs.

And now, *Sir*, that you may not expect in the following Papers ſuch a number and variety of Experiments as I might perhaps be able to preſent you with, on ſome more tractable ſubject; I ſhall briefly mention to you ſome of the chief difficulties I

met with in the making of theſe; which I do the rather, that, if you and your ingenious friends have a mind to proſecute ſuch Tryals, you may not be ſurpris'd with the difficulties I have met with; but provide at leaſt againſt thoſe fore-ſeen ones, by which you will ſcarce fail to be encounter'd.

I ſhall then inform you, that the enſuing experiments were rendred uneaſie and troubleſome to me by this; that ſome of them could not be conveniently done at all ſeaſons of the year, nor in any ſeaſon in all weathers; but muſt be made not only in the day time, but in Sun-ſhine days. You will eaſily gheſs, that I ſpeak of thoſe experiments, that are to be made by the help of a Burning-glaſs, caſting the reflected or refracted beams of the Sun upon the combuſtible matter plac'd in the exhauſted Receiver:

For,

For, by reaſon of the interpoſition of ſo thick a Glaſs, whereby many of the incident beams of Light are reflected, and others inconveniently refracted; there is ordinarily requiſite a clear day, and a competent height of the Sun above the *Horizon*; and ſometimes alſo a convenient time of the year; to bring ſuch experiments, as we were ſpeaking of, to a fair Tryal. Not to take notice, that in ſuch attempts there uſually intervene circumſtantial difficulties, not ſo eaſie to be fore-ſeen: And it not being Summer when I had occaſion to make the following Experiments, I could make but very few with the Sun-beams; beſides that there are divers others which are not that way to be made ſo conveniently, if at all, as by the help of the Fire.

But though the Tryals of this ſecond ſort had their *conveniencies*, in

regard they might be made in any weather, and as well by night, as day; yet they were not unattended with peculiar *inconveniencies*: ſome of which you will eaſily diſcern by the mention of them, that was neceſſary to be made in ſome of the relations themſelves. And, beſides more particular and emergent difficulties, there was this in general, that render'd theſe Experiments troubleſome; that, whether I made them in larger Receivers, or in ſmall, or in middle-ſized ones, each of theſe caſes had its inconveniencies: For, very large Receivers, beſides that 'twas very toilſome and tedious to empty them of Air, required ſo much time for the exhauſtion, that too frequently, by that time the *Operator* had done pumping, the included Iron, or other heated body, was grown too cold to perform the deſired effect: And if the

the Receiver were not conſiderably large, then the red-hot Iron, or other included body that was to burn the combuſtible matter, would much endanger the breaking of the over-heated Glaſs, and not afford room enough for ſome *Phænomena* to be fairly exhibited in; and beſides create another difficulty, to which we found middle-ſized Receivers alſo obnoxious: For, ſeveral times when the Experiment required an intenſe heat within the Receiver, then (eſpecially if ſome caſual obſtacle hinder'd the quick exhauſtion) the heat of the ignited Iron, or ſome ſuch other included body, would ſo melt or ſoften the Cement, that faſten'd the Receiver to the Engine, that, when the Glaſs was brought to be well exhauſted, and ſometimes alſo before, the external air would by its preſſure and fluidity ſqueeze or thruſt in ſomewhere or

other the yielding Cement, and thereby cauſe in the Inſtrument a leak, that would much incommodate us, if not reduce us to begin the Experiment again, in ſo much that for ſome tryals we were fain to provide a Cement on purpoſe: the leaſt fuſible, that we uſed on other occaſions, being yet found too fuſible on theſe.

Nor were thoſe, I have already mentioned, the only difficulties and impediments I met with in making experiments about *Flame* and *Air*; but I ſhall not here trouble you with them in this place, where it may ſuffice for me to have mentioned thoſe that are of a more general nature, and are like the moſt frequently to occurr.

But though I declin'd to name any other to you, than the foregoing difficulties *in making* the following Experiments; yet I muſt not omit to take

take notice of one that may occurr to you about *judging* of them. For, in those tryals that require to have an ignited Iron or any such thing included in the Receiver, it would usually happen, that so much heat would rarifie the Air shut up in the Mercurial Gage, and consequently inable it to depress the Mercury, that lies under it, far beneath the mark it would have staid at upon the meer account of so much ambient Air pump'd out: This would happen, I say, before the heated Receiver was well exhausted; so that, if one be not aware of this, 'twill be obvious, by looking on the Gage, to conclude the Receiver to be well emptied, before it really is so. And therefore the safest way in these cases is, to continue to pump (without trusting to the ordinary marks) till you see that the Mercury will be no further depressed in the sealed leg

of

of the Gage; though otherwiſe, by concurring ſigns, one that is vers'd in thoſe tryals may well enough judge when he needs to pump no longer.

But perhaps you will here demand, whether by our Engine we can competently withdraw the Air out of a Receiver; or whether at leaſt that may not be much better done by the help of Quick-ſilver, after the manner of the *Torricellian* Experiment, in regard that ponderous liquor frees the glaſs, it deſerts, from all the Air at once, and exactly hinders the regreſs of it.

In anſwer whereunto, I hope you do not expect, that I ſhould contend for a favourabler judgement of the Engine, I employ, than the *Virtuoſi* (as well Foreign as Engliſh) have been pleaſed to paſs on it already: And therefore, to tell you freely my thoughts,

thoughts, about the main part of the propos'd Queſtion, I ſhall readily avow to you, that I think, there may be experiments (ſuch as ſome of thoſe where the included body need be but ſmall, and where the being ſuddenly produc'd is chiefly deſired in the effect,) wherein, by the help of Quick-ſilver, the exhauſtion of the Air may be diſpatch'd with greater celerity, and conſequently make the effect be more conſpicuous, than, *by our ordinary way of trying*, it would be in our Engine; ſince the fall of the Mercury does, as the objection intimates, produce a *Vacuum* (in our ſence of that word) very nimbly, whereby the Expanſion of the Air is preſently effected, and the Aereal particles, harboured in the pores of any body plac'd in this deſerted cavity, will thereby have opportunity more ſuddenly to expand themſelves. But,

on

on the other ſide, I might anſwer in general, that when I have particular occaſions to diſpatch the exhauſtion of the Air, I can very much haſten it, by barely leſſening, as I have ſeveral times done, the capacity of the Receiver; inſomuch that I have ſometimes imployed ſo ſmall an one, that in half a minute, or much leſs, after it was fitted on, we could conſiderably exhauſt it, and thereby produce *Phænomena* exceeding conſpicuous. And as to the Experiments of this little Tract in particular, it may be ſaid, that, not to mention the troubleſomneſs, and other inconveniencies of needing to imploy ſuch an unwieldy weight of Mercury, you will eaſily find, by the *Phænomena* of divers of the inſuing tryals, that moſt of them cannot be with any conveniency, and ſome of them not at all, made in the *Torricellian* tubes. As for

for the ground of the Objection, that the Air cannot be ſo well drawn out by our way, as by the ſubſiding of the Mercury; though you may think that very clear, yet one, that were very jealous of the Reputation of the inſtrument I employ, may perhaps reaſonably enough queſtion it. For, the *Vacuum*, that is produced in the *Torricellian* Experiment, as 'tis made all at once, ſo 'tis made once for all; and therefore if there were any Aereal particles lurking in the Mercury (as there will be pretty ſtore, if the quantity of that liquor be great enough to make a conſiderable *Vacuum*; which if it be not, it will be too ſmall for very many of our tryals;) they will remain in the deſerted cavity at the top of the Glaſs, and, by their expanſion there, much hinder the full operation of an ambient *Vacuum* upon the bodies plac'd in it.

Beſides

Beſides that almoſt all ſuch bodies, if they be dry, will be ſo incongruous to Mercury (which ſcarce ſticks to any conſiſtent bodies but metals,) that probably there will be no ſmall number of aereal corpuſcles intercepted between the Mercury and thoſe ſurfaces, to which it does not cloſely adhere: which aery corpuſcles, when the ſubſiding Mercury deſerts them, will be left to increaſe the number of thoſe, that, (as we were ſaying) will emerge from the Mercury; from which, as alſo from the pores of the included bodies, will perhaps ariſe divers new ones from time to time for a pretty while after. And in caſe the *Vacuum* be made by a *Cylinder* of two or three and thirty foot of water, (as for ſome experiments, that have been tried in *France* and *Italy*, hath been done) the emerſion of bubles may laſt a long time, as may be gather'd

ther'd from ſome obſervations of mine elſewhere related.

On the contrary, in our Engine, though when the Receivers are not very ſmall, they are more ſlowly emptied; yet in recompence, we may continue the pumping out of the Air as long, and renew it in the ſame Experiment as often as we think fit: So that, if we perceive, that, after the firſt exhauſtion of the Glaſs, there happen any aereal particles to extricate themſelves ſucceſſively out of the included body, we can, by reſuming the Pump from time to time, whenever need requires, free the *Vacuum* from theſe alſo; which, in ſome caſes, I have found to be longer and more copiouſly emitted by the included bodies, than any thing but jealous tryals could have convinc'd me of. And to confirm what I have been ſaying by ſomething Hiſtorical, I ſhall add;

add; That though the excellent *Florentine Academians* are thought to have profecuted the Experiments about the *Vacuum* made with Mercury the furtheſt of any; yet ſome eminent members of that Illuſtrious Society were pleaſed to confeſs to me, that they never were able by the help of Mercury, to bring a Glaſs-buble, ſeal'd up with Air in it, to burſt of it ſelf by the withdrawing of the external Air; which yet I have often done with the Engine I employ, and convinced them, that I could do ſo by doing it in their preſence.

You will, perhaps, think it ſomewhat ſtrange, to find, that I ſet down ſome of the following Narratives in ſuch a way, as does not expreſs me ſollicitous to aſcribe and vindicate to the Air ſo abſolute and equal a neceſſity to the production and conſervation

vation of all *Flames*, as divers Learned men have concluded from my former Experiments. But I, that am content to be kind to the Air, but not partial, ſhall not ſcruple to declare to you, that, as much as ſome may think me beholden to the Air for any diſcoveries of it ſelf, it may have vouchſaf'd me; yet, I think, a natural, as well as a civil Hiſtorian, does, in his accounts of matters of fact, owe more to Truth than to Gratitude it ſelf. And though, where-ever the Air can challenge a clear, or at leaſt a probable intereſt in a *Phænomenon*, I am not only diſpos'd, but glad to do it right; yet I would not eaſily aſſert to it a larger Juriſdiction than I find Nature to have aſſigned it; eſpecially ſince (without partiality) *that*, I preſume, may be ſhewn to be very large and conſiderable, and perhaps to reach to many things, wherewith

men ſeem not to have yet taken notice that it hath any thing to do at all.

What hath been hitherto ſaid, will not, I hope, ſeem impertinent or uſeleſs, whenever you ſhall fall upon the actual making of ſuch Experiments as you are about to read. But I fear, that to add any thing more, (which were not difficult for me to do to the preliminary part of this ſmall Tract) would make it too diſproportionate to the hiſtorical; From which I ſhall therefore no longer detain you.

THE

THE FIRST TITLE.

OF THE DIFFICULTY OF *PRODUCING* FLAME WITHOUT AIR.

THE FIRST TITLE.

Of the Difficulty of *Producing* FLAME without AIRE.

EXPERIMENT I.

A way of kindling Brimſtone in vacuo Boyliano *unſucceſsfully tried.*

WE took a ſmall earthen melting Pot, of an almoſt Cylindrical figure, and well glaz'd (when it was firſt bak'd) by the heat; and into this we put a ſmall cylinder of Iron of about an inch in thickneſs, and half as much more in *Diameter*, made red hot in the fire; and having haſtily pump'd out the Air, to prevent

the breaking of the Glaſs; when this veſſel ſeem'd to be well emptied, we let down, by a turning key, a piece of Paper, wherein was put a convenient quantity of flower of Brimſtone, under which the iron had been carefully plac'd; ſo that, being let down, it might fall upon the heated metal, which as ſoon as it came to do, that vehement heat did, as we expected, preſently *deſtroy* the contiguous paper; whence the included Sulphur fell immediately upon the iron, whoſe upper part was a little concave, that it might contain the flowers when melted. But all the heat of the iron, though it made the Paper and Sulphur ſmoke, would not actually kindle either of them that we could perceive.

EXPER. II.

EXPER. II.

An ineffectual attempt to kindle Sulphur in our Vacuum *another way.*

ANother way I thought of to examine the inflammability of Sulphur without Air; which, though it may prove somewhat hazardous to put it in practice, I resolved to try, and did so after the following manner:

Into a Glass-buble of a convenient size, and furnish'd with a neck fit for our purpose, we put a little flower of Brimstone (as likely to be more pure and inflammable than common Sulphur;) and having exhausted the Glass, and secured it against the return of the Air, we laid it upon burning coals, where it did not take fire, but rise all to the opposite part of

the glaſs, in the form of a fine powder; and that part being turned downward and laid on coals, the Brimſtone, without kindling, roſe again in the form of an expanded ſubſtance, which (being removed from the fire) was, for the moſt part, tranſparent, not unlike a yellow varniſh.

ADVERTISEMENT.

THough theſe unſucceſsful attempts to kindle Sulphur in our exhauſted Receivers, were made more diſcouraging by ſome more, that were made another way; yet judging that laſt way to be rational enough, we perſiſted ſomewhat obſtinately in our endeavours, and conjecturing that there might be ſome unperceived difference between Minerals, that do all of them paſs, and

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are ſold for common Sulphur, I made trial, according to the way hereafter to be mentioned, with another parcel of brimſtone, which differ'd not ſo much from the former, as to make it worth while to ſet down a deſcription of it, that probably would not be uſeful.

But in this place, it may ſuffice to have given a general intimation of the poſſibility of the thing. The proof of it you will meet with under the *third Title*, when I come to tell you what uſe I endeavour'd to make of our ſulphureous Flames.

EXPER. III.

EXPER. III.

Shewing the efficacy of Air in the production of Flame, without any actually flaming or burning body.

HAving hitherto examin'd by the *preſence* of the Air, what intereſt it has in kindling of Flame; it will not be impertinent to add an Experiment or two, that we tried to ſhew the ſame intereſt of the Air by the effects of its *admiſſion* into our *Vacuum*. For I thought, it might reaſonably be ſuppoſed, that if ſuch diſpoſitions were introduc'd into a body, as that there ſhould not appear any thing wanting to turn it into Flame but the preſence of the Air, an actual accenſion of that body might be produced by the admitted Air,

Air, without the intervention of any actual Flame, or Fire, or even heated ſubſtance; the warrantableneſs of which ſuppoſition may be judged by the two following Experiments.

When we had made the Experiment, ere long to be related in its due place, (*viz. Title* II. Exper. the 2[d]) to examine the preſumption we had, that even when the Iron was not hot enough to keep the melted Brimſtone in ſuch a heat, as was requiſite to make it burn without Air, or with very little, it would yet be hot enough to kindle the Sulphur, if the Air had acceſs to it: To examine this (I ſay) we made two or three ſeveral Tryals, and found by them, that if ſome little while after the flame was extinguiſhed, the Receiver were removed, the Sulphur would preſently take fire again, and flame as vigorouſly as before. But I thought

it

it might without abſurdity be doubted, whether or no the agency of the Air in the production of the flame might not be ſomewhat leſs than theſe trials would perſwade; becauſe that, by taking off the Receiver, the Sulphur was not only expoſed to freſh Air, but alſo advantaged with a free ſcope for the avolation of thoſe fumes, which in a cloſe Veſſel might be preſum'd to have been unfriendly to the Flame.

How far this doubt may, and how far it ſhould, be admitted, we may be aſſiſted to diſcern by the ſubjoyned experiment, though made in great part for another purpoſe; which you will perceive by the beginning of the Memorial I made of it, that runs thus.

EXPER. IV.

EXPER. IV.

A differing Experiment to the ſame purpoſe with the former.

HAving a mind to try, at how great a degree of rarefaction of the Air it was poſſible to make Sulphur flame by the aſſiſtance of an adventitious heat, we cauſed ſuch an experiment as the above mention'd to be reiterated, and the pumping to be continued for ſome time after the flame of the melted flowers of Brimſtone appeared to be quite extinguiſhed, and the Receiver was judg'd by thoſe that managed the Pump (and that upon probable ſigns) to be very well exhauſted. Then, without ſtirring the Receiver, we let in at the ſtop-cock very warily a little Air, upon which we could perceive, though not

not a conſtant flame, yet divers little flaſhes, as it were, which diſcloſed themſelves by their blew colour to be ſulphureous flames; and yet the Air that had ſuffic'd to re-kindle the Sulphur, was ſo little, that two exſuctions more drew it out again, and quite depriv'd us of the mentioned flaſhes. And when a little Air was cautiouſly let in again at the ſtop-cock, the like flaſhes began again to appear, which upon two exuctions more did again quite vaniſh, though, upon the letting in a little freſh Air the third time, they did once more reappear.

Whether and how far ſuch experiments as theſe may conduce to explicate what is related of Fires ſuddenly appearing in long undiſcloſed Vaults or Caves to thoſe that firſt broke into them, I may perchance elſewhere conſider; but ſhall not here inquire,

eſpecially

eſpecially being not yet fully ſatiſfied of the truth of the matter of fact.

EXPER. V.

About an endeavour to fire Gunpowder in vacuo *with the Sunbeams.*

WHatever hath been hitherto deliver'd, will not, I preſume, make it unreaſonable to enquire, whether, what intereſt ſoever the Air appears to have in the production of thoſe flames that are to laſt for ſome time, there may not eaſily be produc'd a momentany flame or flaſh without any aſſiſtance from the Air. Wherefore I employ'd ſome endeavours to diſcover, whether there were the ſame need of Air to the going

off

off of Gunpowder, as to the inflammation of other bodies. And though my first attempt of this nature being unprosperous, it was concluded by the Learned of the by-standers, that I should never be able to make a successful one to kindle Gunpowder in an exhausted Receiver; yet this did not hinder me from prosecuting a design, for whose feasibility I consider'd, that it might be alledged *à priori* (as they use to speak) that Brimstone, which is one of the ingredients of Gunpowder, appears by several tryals to be sometimes capable of accension in our *Vacuum*, and therefore probably may kindle the rest. But how far the firing of Powder, without the help of Air, is *possible*, will be best judg'd by the experiments you will meet with under the *third Title*: And how far 'tis more *difficult* to be kindled in our exhausted Receivers, than

than in the open air (which is an inquiry proper for this place) may be gueſs'd by the ſubjoyn'd tryal; which, though it were made many years ſince (in the year 1660.) before we had devis'd the Mercurial Gage, to examine how well the Receiver was exhauſted, I ſhall yet afford it a room in this place, becauſe 'twas made in Summer by the help of a Burning-glaſs, which I could not imploy to purpoſe in the winter-ſeaſon, wherein the two following Tryals were made.

To give you then ſome account of that part of the Experiment, which concerns our preſent inquiry, I will ſubjoyn a tranſcript of what I find regiſtred about it; which is to this purpoſe, and almoſt in theſe words: That, having conveniently plac'd three or four grains of Gunpowder in the cavity of our Receiver, and ha-

ving carefully drawn out the Air, we caſt the Sun-beams, united by a good Burning-glaſs, upon the powder, and kept them there a pretty while to little purpoſe; till at length the Powder, inſtead of taking fire, ſmoaking only, and melting like a metal; thoſe Spectators, that were of another opinion than I was yet convinc'd of, would have me leave off. The further event of ſuch tryals more fully proſecuted you will find under the *third Title*: All that will be pertinent to be here added being, that the newly recited experiment was not the ſingle one, we made about that time, that diſcover'd a great indiſpoſition even in Gunpowder to be fir'd in our *Vacuum*.

EXPER. VI.

EXPER. VI.

An attempt to fire Gunpowder in vacuo, *by means of a hot Iron.*

WE took (by weight) what we judged a convenient quantity of Gunpowder, that was extraordinarily ſtrong and well made, and having in our Receiver, that was capable of holding about 16 pound of water, placed the formerly mention'd Iron firſt heated red-hot, when the Air appeared by the Mercurial Gage to have been diligently pump'd out, we let down, by help of the turning Key, a ſmall piece of thin Paper, wherein the Powder had been put, till we ſaw it reach'd the plate, by whoſe heat we hop'd the Paper would be deſtroyed, and the Powder made to go off. But though both the one and

the other had been purpoſely well dried near the fire, before they were put into the Receiver; the deſired exploſion of the Powder did not inſue. Yet there appeared upon the iron-plate a pretty broad blew flame, like that of brimſtone (whence 'twas judg'd to be the Sulphureous ingredient of the Gunpowder that was kindled) which laſted ſo very long as we could not but wonder at it. But at length the Powder not going off, and the ſtill decaying heat of the iron forbidding us to wait any longer, we thought fit to take off the Receiver, & found (as we expected) that the Paper contiguous to the Iron, was, in part, deſtroy'd by its heat; but moſt of the grains of the Powder ſeem'd not alter'd, and were found diſpos'd enough to be fired, notwithſtanding the conſumption of the brimſtone, that had burn'd away.

A P-

APPENDIX.

TO confirm the foregoing Experiment, by ſhewing how great a diſpoſition to take fire there may be in Gunpowder, that yet would not do ſo without Air, I ſhall ſubjoyn this obſervation:

Having reiterated the newly mention'd Experiment after the like manner, and with the ſame Receiver, and Iron-plate, as formerly, we did not find any exploſion to be made for ſo long a time, that, thinking it in vain to wait any farther, we let in the Air, which might perhaps, by help of the remaining heat of the iron, procure the operation we at firſt deſired. The event was; that after nothing had inſued for a good while, and we ſcarce thought, that ſuch a thing would happen; the Powder ſuddenly went off

with a great flash, and so shook the Receiver that was yet standing on the Engine, as to endanger the throwing of it down. Which circumstance I mention, to give you a caution that may prove useful, in case you try in close vessels experiments with Gunpowder; since if they be not warily managed, they may sometimes (as I have had occasion to observe) prove dangerous enough; which will be the better discern'd, if I add, that the Powder, that had this operation on a Receiver (large enough to contain two Gallons of liquor) was weigh'd before it was put in, and amounted but to one grain, (though a greater quantity might perhaps have been well enough ventur'd upon, if it had been but *common* Gunpowder.)

EXPER. VII.

EXPER. VII.

Reciting another way, whereby the firing of Gunpowder in vacuo Boyliano *was attempted.*

TO diverſifie our ways of examining the indiſpoſedneſs of Gunpowder to be fired in our *Vacuum*, we thought fit to add to the foregoing Trials that which followeth:

Into a pretty large and ſtrong Glaſs-buble, we put a few ſmall corns of Gunpowder, and having carefully exhauſted it, and ſecur'd it againſt the return of the Air, we put it upon a pretty quantity of Live-coals ſuperficially cover'd with Aſhes: By whoſe heat the ſulphureous ingredient of the Powder was in part kindled, and burn'd blew for a pretty while, and with a flame conſiderably great

(in proportion to the Powder;) upon whoſe ceaſing, the Powder, which, when all was done, did not take fire, appear'd to have ſent up, beſides the flame, a pretty deal of ſulphureous ſublimate, that ſtuck to the upper part of the Glaſs, and being held againſt a Candle, we caus'd to be brought in, (for the Experiment had been purpoſely made in a dark place) it exhibited divers vivid colours like thoſe of the Rain-bow.

EXPER. VIII.

About a tryal made to fire Gunpowder in our Vacuum *by the help of Sparks.*

THough, in the fourteenth of the long ſince publiſh'd Phyſico-Mechanical Experiments, there is recited

recited a tryal made about kindling of Gunpowder with a Piſtol; yet I ſhall not forbear to ſubjoyn the inſuing account, *partly* becauſe the Receiver we then imploy'd, being about four times, if I miſ-remember not, as big as that we laſt made uſe of, 'twas very difficult to exhauſt the one ſo well as the other; and *partly* becauſe we wanted ſome accommodations, with which we ſince furniſh'd our ſelves, and (having not then devis'd the Mercurial Gage we imploy'd in the making this laſt Experiment) we could not then judge ſo well, as we ſince could, of the degrees to which the Receiver was emptied. And therefore, when in the Relation of that fourteenth tryal there is mention made of one attempt that did ſucceed, among divers that did not; there is towards the cloſe an intimation given, That in ſpite of the great Rare-

faction

faction that had been made in the Air, there might yet be ſome little portion of it remaining in the Receiver. I proceed then to the promiſed Relation, which I find thus ſet down:

To proſecute the deſign of the foregoing Experiment by a way ſomewhat differing from thoſe hitherto mention'd; we made, though not without difficulty, the enſuing tryal; one of whoſe ſcopes you will find intimated at the cloſe of the Relation.

We took a ſmall and very ſhort Piſtol, and having well faſten'd it with ſtrings to a great weight, that was plac'd upon the iron-plate of our Engine, we drew up the cock, and prim'd the pan with dry Powder; then over both the weight and Piſtol we whelm'd a Receiver, capable of containing two Gallons of liquor, and having carefully cemented it on, we cauſed the Air to be diligently

pump'd out; having before put in a Mercurial Gage to help us to diſcern when it was exhauſted. Laſtly, ordering the Pump to be plied in the mean while, for fear ſome Air ſhould ſteal in, before the tryal was compleated, we did, by the motion of the turning key, ſhorten a ſtring that was tied both to it and the trigger of the Piſtol, by which means we did as much as we could towards the firing of the powder in the pan; but though the pan were made to fly open, yet the powder did not go off: whereupon letting in the Air, and cocking the Piſtol again without taking it off the weight it was tied to before, we drew out a little Air, to be ſure that the Receiver was cloſely cemented on, (which care we took in reference to another Experiment;) and then letting in the Air at the top of the Receiver, and ſtopping it in with the turning

ing key, we did, by the help of that key, draw aſide the trigger again; whereupon, though there had been no new Powder put into the Pan, nor any left in it, but only ſome little that remain'd after the late tryal; yet that little readily took fire and flaſh'd in the pan; which made it the more probable, that in the former tryal ſparks of fire had been ſtruck out by the colliſion of the Flint and Steel: which was the more credible, becauſe in an other tryal, made the ſame hour in the ſame exhauſted Receiver, two of the aſſiſtants plainly ſaw a ſpark or two fly out upon the falling of the Cock, though I, that chanced to ſtand in an inconvenient place, did not then perceive it. But afterwards, having caus'd the Experiment for my fuller ſatisfaction to be repeated, I freed my ſelf from need of truſting others eyes: So that it appears, that, notwithſtanding

ing the great indiſpoſition of Gunpowder it ſelf to be reduced into flame in our *Vacuum*; yet even ſolid matter is not uncapable of being ignited there, if it be put into a motion ſufficiently vehement.

If this Experiment had not been ſo very troubleſome to make, I ſhould have been invited to reiterate it, becauſe a not contemptible ſcruple may be prevented, if the tryal can be made to ſucceed, in regard that the going off of the whole Gunpowder, by the falling of a ſpark or two only upon two or three of its grains, would argue, that the accenſion of the reſt was made by the propagation of flame from the kindled grains to the reſt; ſo ſmall a portion of ignited and ſuddenly vaniſhing matter, as is to be found in a ſpark or two, being not likely to be able in ſo very ſhort a time to impart a Vehement, or ſo much

much as a Senſible, heat, to the whole aggregate of grains, or at leaſt a great part of them, as the *focus* of a Burning-glaſs, held long enough upon them to make them melt, may well be ſuppoſed to do.

EXPER. IX.

Two ways of making Aurum fulminans *go off in our exhauſted Receiver.*

BEcauſe 'tis wont to be ſuppoſed (how juſtly I here diſpute not,) that *Aurum Fulminans*, as the Chymiſts call it, is much of the nature of Gunpowder, though by vaſt odds ſtronger than it; I thought it not unfit to make tryal, whether it could be made to go off in our exhauſted Receiver; and accordingly, about the time

time that the other Experiment of firing Gunpowder by the Sun-beams was made, we alſo made tryal of this; and that, as I remember, in the ſame Receiver, and with the ſame Burning-glaſs. The event was; that, though the Air had been pump'd out, the concentrated beams of the Sun made the *Aurum fulminans* go off, and violently ſcatter about the cavity of the Receiver a yellowiſh duſt or powder, which other tryals in the free Air made us look upon as particles of the Gold, that was the main ingredient of this odd compoſition.

This Experiment we reiterated a good while after in another place, and with other veſſels, and yet with the like ſucceſs.

But in regard theſe tryals being made by the united Sun-beams, 'twas unavoidable that our eyes would be before-hand affected with the vivid impreſ-

impreſſions of ſo glaring a light; it ſeem'd not ſafe to determine, by the bare going off, or ſhattering of the *Aurum fulminans*, whether or no it afforded any flame or light upon its exploſion: For, as we could not be ſure of the affirmative, becauſe our eyes could not diſcern any momentany flame or flaſh; ſo it ſeem'd not ſafe to conclude the negative: ſince, though there had been ſuch a flame, yet, if it had not been ſtrong, it would not have been ſenſible to our eyes, whilſt preaffected by a powerful Light. Wherefore we reſolv'd to make this tryal in the night with an Iron heated, but not candent, (that its light might not eclipſe that which the powder might afford;) and having, after the manner already often recited, exhauſted a pretty large Receiver, and let down by a ſtring half a quarter of a grain (by weight) of good *Aurum*

fulminans

fulminans of our own preparing, loosely tied in a little piece of thin paper, (which paper, former tryals to another purpose kept us from fearing that no hotter an iron, than ours then was, would kindle,) we found, as we expected, that after the powder had lain long enough upon the iron to be throughly heated, it went off all together, and, as the by-standers affirm'd, with a flash: but my face being accidentally turn'd to remove a Light that I feared might disturb us, I could not see the flash my self, and therefore caus'd the Experiment to be made once more, to ground my narrative upon my own observation: which quickly assur'd me, that the Luminous flash, produc'd upon the explosion, was not only sensible, but considerable.

THE SECOND TITLE.

OF THE DIFFICULTY OF *PRESERVING* FLAME WITHOUT AIR.

Of the Difficulty of Preſerving *Flame without Air.*

SInce it is generally, and in moſt caſes juſtly, eſteemed to be more eaſie to *preſerve* Flame in a body that is already actually kindled, than to *produce* it there at firſt; we thought fit to try, whether at leaſt bodies already burning might not be kept in that ſtate without the concurrence of Air. And though in ſome of our formerly publiſhed Phyſico-Mechanical experiments it happen'd, that actual Flame would ſcarce laſt a minute or two in our *large* Pneumatical Receiver; yet becauſe it ſeem'd not improbable, that Mineral bodies once kindled might

afford a vigorous and very durable flame; we thought fit to deviſe and make the following tryals: Whence probably we might receive ſome new informations about the *Diverſities*, and ſome other *Phænomena* of Flame, and the various *degrees*, wherein the Air is neceſſary or helpful to them.

EXPER. I.

Reciting an attempt to preſerve the flame of Brimſtone without Air.

WE put upon a thick metalline place a convenient quantity of flowers of Sulphur; and having kindled them in the Air, we nimbly conveyed them into a Receiver, and made haſte to pump out ſome of the included Air, partly for other reaſons, and partly that the cavity

cavity of the Receiver might be the ſooner freed from ſmoak, which would, if plentiful, both injure the flame, and hinder our ſight. As ſoon as the Pump began to be plied, or preſently after, the flame appear'd to be ſenſibly decayed, and continued to be leſſen'd at every exſuction of the Air; and in effect, it expir'd before the Air was quite drawn out. Nor did it, upon the early removal of the Receiver, do any more than afford, for a very little while, ſomewhat more of ſmoak in the open Air, than it appear'd to do before.

The reiteration of this Experiment, preſently after, afforded us nothing new, worth mentioning in this place.

EXPER. II.

Relating a Tryal about the Duration of the flame of Sulphur in vacuo Boyliano.

TO vary a little the foregoing Experiment, and try to ſave ſome moments of time, which on theſe occaſions is to be husbanded with the utmoſt care; having provided a Cylinder of iron, larger than the former, that it might by its bulk, being once heated, both contribute to the accenſion of the Sulphur, and to the laſting of its flame, we made a tryal, that I find regiſtred to this effect:

We took a pretty big lump of Brimſtone, and tied it to the turning-key; and having got what elſe was neceſſary in a readineſs, we caus'd the iron-plate to be haſtily brought red-hot

red-hot from the fire, and put upon a Pedeſtal, that the flame might be the more conſpicuous; and, having nimbly cemented on the Receiver, we ſpeedily let down the ſuſpended Brimſtone till it reſted upon the red-hot iron, by which being kindled, it ſent up a great flame with copious fumes, which hinder'd us not from plying the Pump, till we had, as we conjectur'd, emptied the Receiver; which we could not do without withdrawing together with the Air much ſulphureous ſmoak (that was offenſive enough both to the eyes and noſtrils.) But notwithſtanding this pumping out of the Air, though the flame did ſeem gradually to be ſomewhat impair'd; yet it manifeſtly continued burning much longer, than by the ſhort duration of other flames in our Receivers (when diligence is us'd to withdraw the Air from them) one could

could have expected. And eſpecially one time, (for the experiment was made more than once) the flame laſted till the Receiver was judg'd to be well exhauſted; and ſome thought it did ſo ſurvive the exhauſtion, that it went not out ſo much for want of Air, as Fuel; the Brimſtone appearing, when we took off the Receiver, either to have been conſum'd by the fire that fed on it, or to have caſually run off from the Iron, whoſe heat had kept it conſtantly melted.

☞ In caſe you ſhould have a mind to proſecute Experiments of the nature of this and the precedent, it may not prove uſeleſs, if I intimate to you the following Advertiſements. 1. For the red-hot iron above mentioned we thought it not amiſs to provide, inſtead of the melting-pot imploy'd in the firſt experiment, a Pedeſtal (if I may ſo call it) made of a lump of dry'd

dry'd Tobacco-pipe-clay, that the vehement heat of the iron might neither fill the Receiver with the ſmoak of what it lean'd on, nor injure the Engine, if it ſhould reſt immediately upon that: And this Pedeſtal ſhould be ſo plac'd, that the iron may be as far, as you can, from the ſides of the Receiver, which elſe the exceſſive heat would indanger.

2. To the above-mention'd concave iron, that was to receive the Brimſtone, we did for ſome occaſions cauſe to be fitted a thick convex piece of iron, ſhap'd almoſt like a flattiſh Button; which was not to be us'd conſtantly, but upon occaſion, that, being laid red-hot over the melted Brimſtone, it might increaſe the heat, and keep the flame from having ſo broad a *ſuperficies*, whereby it would conſume its fuel too faſt.

3. We ſometimes thought it expedient,

dient, for the clearer diſcerning of what ſhould happen in the Receiver, to make the experiment by night, and remove the Candles when we were juſt about to pump, preſuming that the flame would be conſpicuous enough by its own light; as indeed we found it to be, though its Light were but dimm, conſidering the greatneſs of the flame; whoſe colour, though it did not quite loſe its wonted blewiſhneſs, ſeem'd yet to have received a great and ſomewhat odd alteration.

4. There is one great inconvenience, ſcarce avoidable in this Experiment, *viz.* that the fumes aſcending very copiouſly, do quickly much darken the Receiver, and (if the tryal be long continued) line it with a kind of flowre of Brimſtone, which obſcures it much more (and therefore ought to be carefully wip'd away whenſoever the Receiver is taken off;)

off;) upon which account you will not, I preſume, wonder, if you ſhall find the *Phænomena* of theſe Experiments not always to be the very ſame with what you meet with in this paper: ſince, as 'tis very poſſible that we may not have been able to obſerve things ſo accurately by reaſon of the newly mention'd fumes and flowers; ſo 'tis not impoſſible, that the difference (if there ſhall be any) of other mens Obſervations from ours ſhould proceed from the ſame cauſe.

Before we paſs from this ſecond Experiment, it will not be amiſs to take notice, that though the flames of Brimſtone may be allow'd to be ſomewhat more durable than the flames of Vegetables are wont to be; yet 'tis not ſafe to conclude, that 'twas meerly upon the account of their native vigour, that the flames above mention'd laſted ſo long in our Receiver.

For

For we ſeem'd to obſerve, that there was requiſite a very intenſe heat of the Iron to make the Sulphur capable of flaming on it, when any conſiderable proportion of Air was withdrawn. For which reaſon it ſeems expedient, according to what I lately intimated, that the Iron, that is to keep it melted, be of a good thickneſs, that it may the longer retain a competent heat; and we thought, it contributed to the ſucceſsfulleſt tryals we made, that in them we us'd, beſides the concave iron, the convex one mention'd in the ſecond Note.

EXPER. III.

EXPER. III.

Of the lasting of the flame of a Metalline substance in the same Vacuum.

THose Sulphurs that Chymists call Metalline, being supposed by many to be of a much more fix'd nature than common Sulphur, and it being indeed probable enough, that in them good store of very minute particles are crowded together, I thought fit to try, whether a body, wherein a vulgar Chymist would think the Sulphur of a metal to be the main ingredient, would afford in our *Vacuum*, a more vigorous or lasting flame than that of common Sulphur. And though I will not here trouble you with my particular scruples about the Chymists doctrine concerning

ing metalline Sulphurs, nor with the grounds on which I devis'd the following inflammable ſolution of *Mars*, (for I do not now give it a more determinate name) which ſome Chymiſts will not perhaps diſlike; I ſhall here annex the enſuing tranſcript of the Tryal it ſelf.

Having provided a ſaline Spirit, which by an uncommon way of preparation was made exceeding ſharp and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of filings of Steel, which were not ſuch as are commonly ſold in ſhops to Chymiſts and Apothecaries, (thoſe being uſually not free enough from ruſt;) but ſuch as I had a while before caus'd to be purpoſely fil'd off from a piece of good ſteel. This metalline powder being moiſtn'd in the viol with a little of the menſtruum,

was

was afterwards drench'd with more; whereupon the mixture grew very hot, and belch'd up copious and ſtinking fumes; which whether they conſiſted altogether of the vol tile ſulphur of the Mars, or of metalline ſteams participating of a ſulphureous nature, and join'd with the ſaline exhalations of the menſtruum, is not neceſſary to be here diſcuſs'd. But whenceſoever this ſtinking ſmoak proceeded, ſo inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a blewiſh and ſomewhat greeniſh flame at the mouth of the viol for a good while together; and that, though with little light, yet with more ſtrength than one would eaſily ſuſpect.

This flaming Viol therefore we conveigh'd into a Receiver, which he, who us'd to manage the Pump, af-

firm'd that about ſix exſuctions would exhauſt. And the Receiver being well cemented on, upon the firſt ſuck the flame ſuddenly appear'd four or five times as great as before; which I aſcrib'd to this, That, upon the withdrawing of the Air, and conſequently the weakning of its preſſure, great ſtore of bubbles were produc'd in the menſtruum, which breaking could not but ſupply the neck of the Viol with ſtore of inflammable ſteams, which (as we thought) took not fire without ſome noiſe; upon the ſecond exſuction of the Air the flame blaz'd out as before, and ſo it likewiſe did upon the third exſuction, but after that it went out; nor could we re-kindle any fire by haſtily removing the Receiver; only we found, that there remained ſuch a diſpoſition in the ſmoak to inflammability, that holding a lighted candle to it, a flame was quickly re-kindled.

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EXPER. IV.

Of the Duration of the Flame of Spirit of Wine impregnated with a metal in the Exhauſted Receiver.

BEcauſe it may, upon grounds not improbable, be thought, that well-dephlegm'd Spirit of Wine being a pure Æthereal liquor, which does not, like combuſtible Sulphurs (whether vulgar or metalline) emit any viſible ſmoak to ſtifle the flame (into which it may in the free air be totally reſolved;) if this ſpirituous and thus qualified liquor could be duly aſſociated with a metalline body, the reſulting flame might be more than ordinarily vigorous and durable; I reſolv'd to make an Experiment of this ſort, and having by a way, that

I deliver in another *paper*, [in a Paradox about the *Fuel of Flames*] ſo united highly rectified ſpirit of Wine with a prepared metal, that they would both afford a conſpicuouſly tincted Flame; we put this mixture into a ſmall Glaſs-lamp, made on purpoſe, and furniſh'd with a very ſlender wieke, which the mixture would not burn whilſt there was liquor enough to imbibe it well; and putting this lighted Lamp into a convenient place of a Receiver that was not ſmall, ſince it was able to contain about two gallons or ſixteen pound of water, we made haſt to cement on the glaſs to the Engine, and yet found not in two or three ſeveral tryals, that, after the Pump began to be moved, ſo little a quantity of tincted flame in that capacious Glaſs laſted much (if at all) more than half a minute of an hour (eſtimated by a minute watch.) And,

And, becauſe the Receiver, we then made uſe of, ſeem'd to me, by reaſon of its ſize and ſome accommodations that belong to it, proper enough to be imploy'd about other tryals, concerning the relation between Flame and Air; I thought fit to try with the ſame ſmall Lamp and liquor, what other *Phænomena* of that kind would be afforded by letting Air in and out, according to the various exigencies of my particular aimes.

But not having then, nor in ſome time after, the leiſure and opportunity of ſetting down things circumſtantially, I contented my ſelf to take thoſe ſhort Notes (of the Principal things) whereof I now ſubjoyn the tranſcript.

When the flame began to decay, the turning key being now and then drawn almoſt out, the tinged flame laſted once a minute

and a half, and another time longer.

The turning key being taken out in the beginning, the flame lasted two minutes or better.

A Pipe bedded in the cement at the bottom of the Glass, and having at each end an open orifice almost of the bigness of that filled by the turning key, which key was then removed from the top; the tincted spirit seem'd to burn very conveniently, as if the flame would have burn'd very long, if we would have permitted it so to do.

The orifice at the top being stop'd with the turning key, though the Pipe were left open at the bottom, it plainly in a short time seem'd much to decay and ready to expire; whereupon I caus'd one to blow constantly, yet but very gently, in at the pipe with a pair of bellows, and by this means, though

though we did not keep the flame vigorous, yet we kept it alive for above four minutes; and then obſerving it to be manifeſtly ſtronger than it was when we began to refreſh it with the Bellows, we ceas'd from blowing, and found, that though the Glaſs-pipe was ſtill left open, yet within about one minute the flame was quite extinguiſhed.

EXPER. V.

Of the Conſervation of Flame under water.

THe better to examine the neceſſity of Air to Flame, I thought fit not only to make the ſeveral Tryals mention'd in this Paper, whether it would live in a *medium* much thinner than *Air*; but alſo to try, whether

it would be able to continue in a *medium* many hundred times thicker than Air, namely in *Water*.

I doubted not but many would think this both an eaſie and a needleſs Inquiry, ſince eminent Writers, both Ancient and Modern, tell us without ſcruple, that *Naptha* and *Camphire* will burn under Water; but I had never the good fortune to be able to make them do ſo; and may be allowed to doubt, whether theſe Writers, notwithſtanding their confidence, deliver what they affirm upon Experience, not bare Tradition. And though in celebrated Authors I have met with divers Receipts of making Compoſitions that will not only burn under water, but be kindled by it; yet I have found thoſe, I had occaſion to conſider, to be ſo lamely or ſo darkly (and ſome of them I fear ſo falſly) ſet down, that by the following compoſition, how

ſlight

ſlight ſoever it may ſeem, I have been able to do more than with things they ſpeak very promiſingly of; ſince, though 'twill not be kindled *by* water, yet being once kindled it will continue to burn *under* water.

And that there might be no ſuſpicion, that whilſt the mixture continued under water, it did only as it were vehemently ferment, or ſuffer a violent agitation of its parts without having them kindled, till in their aſcending they were actually fired by the contact of the air, incumbent on the ſurface of the water; To obviate this ſuſpicion (I ſay) we were careful to try the Experiment, not only in other Veſſels, but in a large Glaſs, the tranſparency of whoſe ſides, as well as that of the contained water, would permit us to ſee for a while the burning of our compoſition, which was ſometimes with a weight detain'd, and

ſome-

ſometimes with a *Forceps* held, till 'twas conſumed, a good way under the ſurface of the Water.

The way of making the Experiment is this: We took of Gunpowder three ounces, of well burnt Charcoal one drachm, of good Sulphur or flower of Brimſtone a little leſs than half a drachm, of choice Salt-petre near a drachm and half: Which Ingredients being well reduc'd to powder, and diligently mingled without any liquor; *either* a large Gooſe-quil, whoſe feathery part was cut off, *or* a piece of a Tobacco-pipe of two or three inches long, and well ſtop'd at one end, had its cavity well fill'd with this mixture, (inſtead of which, beaten Gunpowder alone might ſerve, if it did not operate too violently, or waſte too ſoon:) For the kindling whereof, the open orifice of the Quil or Pipe was carefully ſtopt with a con-

convenient quantity of the ſame mixture, made up with as little Chymical Oyle or Water, as would bring it to a fit conſiſtence. This Wild-fire was kindled in the Air, and the Quill or Pipe, together with a weight, to which 'twas tied to keep it from aſcending, was ſlowly let down to a convenient depth under water, where it would continue to burn, as appeared by the great ſmoak it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the Quil or Pipe kept the dry mixture from being acceſſible to the water (that would have diſorder'd and ſpoil'd it) at any other part than the upper Orifice; and there the ſtream of kindled matter iſſued out with ſuch violence, as did inceſſantly beat off the neighbouring water, and kept it from entering into the cavity that contain'd the mixture, which therefore would continue burning till 'twas conſumed. 'Tis

'Tis probable, that moſt men will conclude from this Experiment, that Air is not ſo abſolutely neceſſary to the duration of Flame, as ſome other of our Tryals ſeem to argue; and that there ought to be a difference made between ordinary Flames, and thoſe that burn with an extraordinary vehemency. But my deſign being, as I long ſince intimated, rather to relate Tryals than debate Hypotheſes, I ſhall only add, that it may be pretended on the behalf of the opinion that this experiment ſeems to diſprove, that, not to mention the Air that may lurk in the Pores of the Water, or that which may be intercepted between the little grains of Powder whereof the mixture conſiſts, the Salt-petre it ſelf may be ſuppos'd to be of ſuch a texture, that in its very formation the corpuſcles, that compoſe it, may intercept ſtore of little aereal

particles

particles between the very minute ſolid ones which thoſe Corpuſcles are made up of. And this inexiſtence of the Air in Nitre may be probably argued from the great windineſs of the flame that is produced upon the deflagration of Nitre. According to this ſurmiſe, though our mixture burns under water, yet it does not burn without air, being ſupplied with enough to ſerve the turn by the numerous eruptions of the aereal particles of the diſſipated Nitre it ſelf.

On this occaſion I remember, that in another Paper I relate, that for divers purpoſes, and among them to remove this ſuſpicion, I ſucceſsfully tried to reproduce Nitre in *Vacuo Boyliano*, that there might not be any Air, or at leaſt any quantity worth heeding, intercepted between the convening particles, that by their coalitions made up the nitrous Corpuſcles,

puſcles, which in favour of the neceſ-ſity of Air to Flame may be pretended to be but ſo many little empty bub-bles cloſe ſtop'd, whoſe moiſter parts may by the fire that kindles the nitre be exceedingly rarified, and in that eſtate emulate air, and violently burſt their little priſons, and throw about the fragments of them with force, and in numbers enough to make their ag-gregate appear ſuch a flame, as is wont to be made by unctuous and truly combuſtible Bodies; and yet this ra-rifi'd ſubſtance, that thus ſhatters the nitrous particles, may really be no true and laſting air, but only vehe-mently agitated vapours, which pre-ſently, upon the ceſſation of the heat, return to liquor; as we ſee, that the vapours of an Æolipile, that iſſue out after the aereal Particles have been expell'd, *though* they make a great noiſe and a temporary wind near the hole

hole they ſtream out at, and would perhaps, if that hole were cloſe ſtopt, break the Æolipile; *yet* are not true and permanent Air, but at a ſmall diſtance off the Inſtrument return into water.

But though I could ſuggeſt other ſuſpicions and conjectures about the incluſion of Air between the particles of Salt-petre; yet I forbear to mention them in a Writing deſign'd to be chiefly Hiſtorical.

EXPER. VI.

Relating an odd Phænomenon *about the Flame of a Metal in our* Vacuum.

TO the foregoing Experiments made on purpoſe, I ſhall add a *Phænomenon* afforded us by chance, and

and yet not unworthy to accompany the reſt.

Whilſt we were trying to kindle ſomething in our exhauſted Receiver, it happen'd by ſome accident or other, that the combuſtible ſubſtance, that was to be kindled, fell beſides the iron, whereby our intended trial was defeated. But whilſt we were conſidering what was to be done on this occaſion, and had not yet let in the Air that had been pump'd out, the lights alſo continuing yet removed; we were ſurpriz'd to ſee ſomething burn like a pale blewiſh Flame almoſt in the midſt of the cavity of the Receiver, and at firſt ſuſpected it to be ſome illuſion of the eyes; but all the by-ſtanders perceiving it alike, and obſerving that it grew very broad, we look'd at it with great attention, and found it to laſt much longer, than, I remember, I have ſeen any flame do in

in an Exhauſted Receiver. I ſhould have ſuſpected, it had proceeded from ſome Brimſtone, ſticking, without our heeding it, to ſome part of the iron, which we had formerly imployed to kindle Sulphur in our Receiver, had it not been that, beſides other things, I remembred, that we had juſt before kept it red-hot in the fire, and conſequently muſt have burn'd away any little Brimſtone, if there were any that adher'd to it: But though we much wonder'd, whence this our Flame proceeded, I would not let any thing be done that might haſten its extinction; and at length, when it expired of it ſelf, we let in the Air, which had been till then kept out, and perceiv'd upon the concave part of the iron (which we judg'd to be the place where the flame had appeared) a piece of melted metal, which we concluded had been faſten'd to the ſtring, that the

fewel we deſign'd to kindle had been tied to, in order to the letting it down the more eaſily: And this made us conceive, that the ſtring happening to be burn'd by the exceſſive heat of the iron, the piece of metal fell into the cavity of it, and, by the ſame heat, the more combuſtible part, which the Chymiſts call the Sulphur, was melted and kept on fire, and continued burning ſo long as we have related. The piece of metal was judg'd to be Lead, but having not formerly obſerv'd ſuch a diſpoſition in that metal to be inflam'd, I conſider'd it attentively, and perceived, that 'twas ſome Fragment, that the Operator had chanced to light on, of a mixture of Lead and Tin that I had (a while before, for an Experiment not at all belonging to our preſent ſubject) cauſed to be colliquated in a certain proportion. Upon whoſe account

it

it ſeems, the mixture of the ingredients had acquired ſuch a new texture, as, whether by making the bodies open one another, or by what other means ſoever, fitted the maſs to afford us the *Phænomenon* above recited. And though I made an unſucceſsful Tryal with a mixture of Lead and Tin to produce ſuch a flame upon the heated Iron in the open Air; yet the newly related experiment may ſuffice to argue, that there may be Flames of metalline Sulphurs (as the Chymiſts call them) that will be at leaſt as eaſily produced without the concurrence of the Air, as that of common Sulphur, and continue to burn in our *Vacuum* longer than it.

THE THIRD TITLE.

Of the ſtrangely Difficult

PROPAGATION

OF

Actual Flame

IN

VACUO BOYLIANO.

THE THIRD TITLE.

Of the ſtrangely difficult Propagation of Actual Flame in Vacuo Boyliano.

I Have more than once obſerv'd, that ſome bodies (whereof I make particular mention in another Paper) though they will not be turn'd into Flame by very intenſe heats, and thoſe of very differing kinds, are yet very readily kindled by an actual Flame. So that the Propagation of Flame to contiguous bodies, that, according to the hitherto obſerv'd and unqueſtion'd courſe of things, muſt thereby in a moment, as it were, be actually inflam'd, ſeems to be not only very

easie, but almost infallible: And yet, that this propagation is not easie, or is perhaps scarce possible to be perform'd without the assisting presence of the Air, may be gather'd from the next following Experiments: At whose titles though you will probably be surpris'd, in regard that by the two first Experiments of the first Title of this Tract it will scarce be expected, that Sulphur should be kindled in our *Vacuum*; yet I presume your wonder will cease, when I put you in mind, that I formerly took notice to you of my having sometimes met with such Sulphur, as would be kindled there; and 'twas, whilst that well-disposed parcel of Sulphur lasted, that I took the opportunity of making with the flame of it the Tryals, to which I now proceed.

EXPER. I.

EXPER. I.

An ineffectual attempt to make Flame kindle Spunck *in an Exhausted Receiver.*

HAving placed the often mention'd Cylindrical plate of iron, first brought to be red-hot, in a Receiver, capable of containing two Gallons of water; and having also diligently pump'd out the Air, we kindled a little Sulphur upon the heated plate, and then a piece of dry'd *Spunck*, tied to a string, was, by the help of a turning key, let down to the Flame; and when the Experiment was finished, and the Spunck was taken out, we found it in divers places not manifestly alter'd so much as in colour; and in those parts, that had been most exposed

posed to the Flame, it was turn'd to a substance very differing from ashes, being black and brittle as tinder, and, like it, exceedingly disposed to be kindled upon the touch of Fire.

EXPER. II.

An unprosperous attempt to make Flame kindle Camphire *without the help of Air.*

AS a farther confirmation of the difficulty of propagating Flame in our *Vacuum*, we may annex the following Tryals.

Into the lately mention'd Receiver we conveyed the Cylindrical plate of iron, made use of in the former Experiment; and when the Air had been diligently pump'd out, we did, by the help of the turning key, let down upon

upon the hot iron a piece of ſuch brimſtone, as would, in ſpite of ſo diſadvantagious a place, be kindled with that heat. A little above this Sulphur we had tied to the ſame ſtring a piece of *Camphire*, that being a body exceedingly apt to take fire, if not (as it were) to draw it, at the flame of lighted Brimſtone. But our Sulphur, melting with the heat of the iron *Cylinder*, dropt unluckily from the ſtring 'twas faſten'd to before, and for the moſt part fell off. And as ſoon as it came to the ground, where it was diſtant from the vehement heat of the metal, the flame expired, and that part of the Sulphur, that happen'd to ſtick to the ſide of the iron, was inflam'd by it: And I, that chanc'd to be then in an inconvenient poſture for ſeeing the Camphire, could not, becauſe of the ſmoak of the extinguiſh'd Brimſtone, well diſcern what

what became of it. But my *Amanuenſis*, that happen'd to be on the beſt ſide of the Receiver, affirm'd, he plainly ſaw the Flame of the Brimſtone reached the Camphire, without being able to make it flame. Which ſeem'd the leſs to be doubted of, becauſe the Camphire was by help of the turning Key let down low enough, and if it had afforded a flame, the difference of Colours betwixt that and the blew flame of Sulphur would have made it very eaſie for me to have diſtinguiſh'd them.

Another tryal I would have thoroughly made to kindle one piece of Sulphur in our *Vacuum* by the flame of another, tied a little lower in the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire; but, though we could find nothing that was viſibly amiſs in the kind of Sulphur we then us'd, yet we were

were not able even by a reiterated tryal to make it take fire upon the iron, where nevertheleſs it melted and ſeem'd a little to boil.

A third Tryal was not ſo unſucceſsful; for having in the well-exhauſted Receiver let down upon the very hot iron a match, made of a piece of Card dipp'd in Brimſtone, the lower extream of it was kindled by the contact of the hot iron. But though the ſulphurated part of the match thus flamed away, yet the remaining part, which was a meer piece of Card, was not thereby turn'd into flame, nor in moſt places ſo much as ſenſibly ſcorch'd or black'd; though (as I remember) the match had been purpoſely dry'd before-hand to facilitate its inflammation.

EXPER. III.

EXPER. III.

A ſtrange Experiment upon Gunpowder, *ſhewing, that though it were fired it ſelf, yet would not fire the contiguous grains in* Vacuo Boyliano.

THe preceding Tryals may ſuffice to manifeſt the difficulty of communicating Flame, without the help of Air, from one body to another, even when the bodies to be kindled are of a very inflammable nature. But becauſe there is no propagation of Flame made in any bodies that we converſe with here below, with any thing near ſuch Celerity as in the contiguous grains of *Gunpowder*; a great heap whereof will, almoſt in the twinkling of an Eye, be turn'd into Flame by propagation from any one ſmall

ſmall kindled grain; nothing ſeem'd fitter to manifeſt how much Flame is beholden to Air, than if ſuch an Experiment could be made, as might ſhew, that, even amongſt the contiguous grains of kindled Gunpowder, Flame would not be propagated without the help of Air. How far a tryal of this nature may be made in our Engine, the following Narratives will beſt declare.

We took ſome Paper, and laying it upon ſome convenient part of the plate of the Engine, we made upon it a train of dry Powder as long as the glaſs would well cover; then, carefully faſtening on the Receiver with good Cement, we ſolicitouſly pump'd out the Air; which done, we took a good Burning-glaſs, and about noon caſt the Sun-beams through it upon the train of ſome Gunpowder: where,

where, though the indiſpoſition to accenſion was ſo great, that the powder did not only ſmoak but melt without going off, and the *Operator* (though vers'd in ſuch Experiments) would not allow that it would ſignifie any thing to continue the tryal any longer; yet upon my being obſtinate to proſecute it, he, being willing to follow the Experiment, rationally conſider'd, that the Receiver, we had been hitherto fain to uſe, was ſo opacous as to reſiſt the entrance of many of the beams that ſhould have their operation upon the Powder: whereupon taking a finer glaſs that was lately come in, we laid by the former, and imploy'd that, which, by reaſon of its tranſparency, ſo little weaken'd the beams of the Sun, that being according to my direction held *obſtinately* upon the ſame parts of the train, they were able to fire ſeveral of them

one

one after another. But though the Sun could thus kindle the Powder, yet it could not make the flame propagate, but only thoſe parts that were melted did at length kindle and fly away, leaving the reſt unalter'd, as I curiouſly obſerv'd, finding ſeveral little maſſes of colliquated matter in ſeveral places of the train, with the Powder unchang'd in all the other parts of the ſame train that lay in a direct line; beſides that ſome of the little colliquated maſſes were contiguous to the reſt of the Powder, which appear'd unchang'd, and kindled readily, and flaſh'd all away as ſoon as I caus'd the Burning-glaſs to be apply'd to it in the open Air.

EXPER. IV.

Reciting another Attempt to confirm the former.

FOr further Confirmation of ſo odd an Experiment, I ſhall alſo add a ſhort account of another made with Gunpowder in our *Vacuum*.

To try on an occaſion, that need not here be diſcours'd of, whether by the help of one of thoſe little inſtruments that are now us'd at *London* to examine the ſtrength of Powder, we could find any difference made by the abſence and preſence of the Air, in the reſiſtance of the Inſtrument, or the effects of the Powder on it; we faſten'd it to a competently heavy and commodiouſly ſhap'd weight of Lead, and when 'twas carefully fill'd and prim'd with Powder, we plac'd it in a Receiver

ceiver of a convenient bigneſs, whence we pump'd out the Air after the uſual manner, and perhaps with more than uſual diligence. But though at length, after the Powder had long reſiſted the beams of the Sun concentrated on it by a good double convex Burning-glaſs, it did, as I expected, take fire at the Touch-hole, and fill the Receiver with ſmoak; yet this kindled Powder could not propagate the flame to that which was in the box, how contiguous ſoever the two parcels were to one another. And when the inſtrument was taken out into the Air, (by which it appear'd how free the Touch-hole was,) as ſoon as ever new-priming with the ſame ſort of Powder was put to it, the whole very readily went off: And when, for further ſatisfaction, we caus'd the inſtrument to be new charg'd, and upon its taking fire only at the Touch-hole in

the exhausted Receiver, we ordered new-priming to be added without so much as taking the instrument out of the Receiver, though afterwards the Receiver was closed again, but without being exhausted of Air; the Powder, though closely shut up in the Glass, did readily go off, as well that which was in the box or cavity of the Powder-tryer, as that which lay on the outward part of the Instrument. And this tryal, for the main, was repeated with the like success.

EXPER. V.

EXPER. V.

Briefly mentioning two differing Tryals with two differing Events, to kindle Gunpowder in our Vacuum.

YOu will eaſily believe, that the event of the foregoing tryals ſeem'd ſtrange enough to the ingenious perſons, that I had deſir'd to be preſent at them; and perhaps the attentive conſideration of it may well enough ſuggeſt ſuch odd ſuſpicions and conjectures, as I have neither the leiſure nor the boldneſs to diſcourſe of in this place.

But here I ſhall not diſſemble my having, by a ſomewhat differing way, made a couple of tryals, whereof though the firſt may confirm the great indiſpoſition of Guhpowder to be

kindled in our *Vacuum*, yet the ſecond ſeems to look another way.

The firſt is ſummarily ſet down in my Notes to this purpoſe. [A few ſmall corns of Gunpowder, being included in a very ſmall buble freed from its Air, and ſecur'd againſt the return of it, or any other, and then apply'd warily to Coals cover'd with Aſhes; did not go off nor burn, but afforded a little yellow powder that ſeem'd to be Sulphur, and ſublim'd to the upper part of the glaſs.]

The Latter's event I found in the ſame paper to have been thus regiſter'd. [But two larger Bubles though ſtrong, whereof one had the Air but in *part*, and the other *carefully* emptied; being provided each of them with a greater quantity of Powder (though ſcarce enough to promiſe ſuch an effect) a while after they were put upon quick Coals, each of them was

blown

blown in pieces with a Report almoſt like that of a Musket; but, though this was done in a dark place, yet we did not perceive, whether or no there were any real flame produc'd.]

The event of this Tryal ſeems at firſt ſight to contradict the inference, that probably you have drawn from the foregoing Experiments. But yet it may not be unworthy of our inquiry, whether this way of tryal be as proper to give ſatisfaction to the curious, as that, made with the Sunbeams, was. And I leave it to be conſider'd, whether or no it may not be doubted, whether the going off of the Gunpowder was caus'd by a ſucceſſive, though extreamly ſwift, propagation of real Flame, from the firſt kindled grains to the reſt; or did not proceed from this, That the coals acting ſtrongly at the ſame time on the whole *Area* or extent of the pow-

der that was next to them, and this in the abſence of the Air, each grain was in that caſe, as 'twere, a little *Granado*, and the heap of them being uniformly enough acted on by the fire, they were made to go off, as to ſenſe, all at once, as if there had been but a contemporary Exploſion made of them all together by the action of the external fire, rather than any true Accenſion made by the flaming grains of the unkindled ones. As I remember I have tried, that even in the open air one may, with a Burning-glaſs dextrouſly imploy'd, make ſome part of a little parcel of *Aurum fulminans* go off, whilſt the neighbouring parts of the ſame parcel, to which the *focus* does not extend with heat enough, will not be made to do ſo.

NEW

NEW EXPERIMENTS

About the Relation betwixt

AIR

AND THE

FLAMMA VITALIS of Animals.

(Sent to the ſame Perſon to whom the former Papers were addreſs'd.)

NEW EXPERIMENTS

About the Relation betwixt Air, *and the* Flamma Vitalis *of Animals.*

(Sent to the ſame Perſon to whom the former Papers were addreſs'd.)

THE xx. Experiments hitherto ſet down under the three foregoing *Titles*, by ſhewing the Relation betwixt Air and Flame in general, may be ſerviceable to the Inquirers into the nature of the *Vital Flame* in particular. But yet having had occaſion to make ſome tryals, that more directly regard the requiſiteneſs of Air to the *Flamma Vitalis* or Vital Prin-

Principle of Animals; I ſhall now preſent you by themſelves as many as I could light on, without being ſolicitous that they ſhould be quite differing from each other; becauſe in ſo new and nice a ſubject, the affinity that may be found between ſome, either in regard of the ſubjects expos'd to tryal, or in the manner of making it, may be uſeful, if not neceſſary, to confirm things by the reſemblance of Events, or make us proceed cautiouſly and diſtinctly in pronouncing upon caſes where the ſucceſs was not uniform.

EXPER. I.

EXPER. I.

Where in the Durations of the Life of an Animal, and of the Flame of Spirit of Wine, included together in a close Vessel, were compared.

WE took some highly rectified Spirit of Wine, and put about a spoonful of it into a small Glass-lamp, conveniently shap'd and purposely blown with a very small orifice, at which we put in a little Cotton-wieke, which was but very slender.

We also provided a tall Glass-Receiver, which was in length 18 inches, and contain'd above twenty pints of water. This Receiver, which was open at both ends, was at the upper orifice (which was not wide) covered

vered with a Brass-plate, fastned on very close with good cement, for uses whose mention belongeth not to this place; and for the lower orifice, which was far the widest, we had provided a Brass-plate furnished with a competent quantity of the cement we imployed to keep the Air out of the Pneumatical Engine; by means of which plate and cement we could sufficiently close the lower orifice (though a wide one) of our Receiver, and hinder the Air from getting in at it.

These things being thus prepared, we took the small Glass-lamp above mentioned, and having lighted it, we plac'd both it and a small Bird (which was a Green-finch) upon the Brass-plate, and in a trice fastned it to the lower orifice of the Receiver, and then watched the event; which was, that within two minutes (as near as we could

could eſtimate by a good minute-watch) the flame, after having ſeveral times almoſt quite diſappear'd, was utterly extinguiſhed; but the Bird, though for a while he ſeem'd to cloſe his eyes as though he were ſick, appear'd lively enough at the end of the third minute; at which time, being unwilling to wait any longer by reaſon of ſome avocations, I cauſed him to be taken out.

After he had for a pretty while, by being kept in the free Air, recovered and refreſhed himſelf, the former tryal was repeated again, and at the end of the ſecond minute the flame of the Lamp went out; but the Bird ſeem'd not to be endanger'd by being kept there a while longer.

After this, we put in together with the ſame Bird two lighted Lamps at once, (*viz.* the former and another like it) whoſe flames, according to expecta-

expectation, lasted not one whole minute before they went out together. But the Bird appear'd not to have been harmed, after having been kept five or six times as long before we took off the Receiver.

In the tall Receiver above-mentioned we included a Mouse, with a lighted Lamp filled with the Spirit of Wine; but before the Experiment was near finish'd, the Mouse, being at liberty within the Glass, made a shift to blow out the flame; which being revived without taking out either the Lamp or the Animal, the Spirit of Wine burn'd about a minute longer, during which time the Mouse appear'd not to be grown sick, no more than it did afterwards, when for some minutes, after the extinction of the flame, he had been kept in the same close and infected Air.

Afterwards

Afterwards we plac'd the ſame Mouſe in another Receiver, which ſeem'd to be by a third part leſs capacious than the former, and in it we alſo fixed a piece of ſlender Wax-candle, ſuch as is wont to be made up in Rolls, (and imployed to light Tobacco.) This Candle continued burning in this new Receiver but for one minute, during which time it emitted ſtore of ſmoak; but this not hindering the Animal to appear lively enough, even after we had kept him much longer in that infected Air, the ſame Candle without being taken out was lighted again, but burn'd not ſo long as before; yet it ſufficed to darken the Receiver, and therefore probably much to clog the included Air, in which nevertheleſs the Mouſe being kept, by our gueſs, eight or ten minutes longer, he appeared, neither when he was taken out, nor a while

before, to have received any considerable harm by his detention there.

EXPER. II.

Of the Duration of the Life of a Bird compar'd with the lasting of a burning Candle and Coal in our Vacuũm.

WE took a Green-finch and a piece of Candle of twelve to the pound, and included them in a great capp'd Receiver, capable of containing about two Gallons or sixteen pound of water, which was very carefully cemented on to the Pump, that no Air might get in or out. In this Glass we suffer'd the Candle to burn till the flame expired, (which it did, in more than one Tryal, within two minutes or somewhat less;) at which

which time the Bird ſeemed to be in no danger of ſudden death; and, though kept a while longer in that clogg'd and ſmoaky Air, appeared to be well enough when the Receiver was removed. Afterwards, we put the ſame Bird into the Receiver with a piece of a ſmall wax Taper, whoſe flame though it laſted longer than the other, yet the Bird outlived it; and 'twas judged he would have done ſo, though the Flame had been much more durable. After this, we included the ſame Bird with the firſt-mention'd Candle in the Receiver, which we had cauſed to be often blown into with a pair of Bellows, to drive out the ſmoak and infected Air; and then beginning to pump out the Air, we found, that the Flame began more quickly to decay, and the Bird to be much more diſcompoſed than in the former Experiments; but ſtill

the *Animal* outlived the Flame, though not without Convulsive motions. The Experiment we repeated with a piece of the fore-mention'd Taper, and the same Bird, which, though cast into threatning symptoms upon the gradual withdrawing of the Air, outlived not only the Flame, but the smoak too that issued from the kindled Wieck; which circumstance was also observed in the preceding Tryal.

Lastly, having freed the Receiver from smoak, and supplyed it with fresh air, we put in with the same Bird a piece of Charcoal of about two inches in length, and half an inch in breadth, which had been, just before 'twas put in, well blown with a pair of Bellows, that it might be freed from ashes, and thorowly kindled; and made haste to pump out the air. This diligence was continued not only till none of the fire could be discern'd by

by any of the By-ſtanders, but till, in our eſtimation, (which the event juſtified) it was irrecoverable by the admiſſion of the outward air; which at its coming in found the Bird very ſick indeed, but yet capable of a very quick Recovery. And this Experiment was, with the ſame Animal and Coal re-kindled, tried over again with the ſame ſucceſs.

Whether this ſurvival of Animals, not only to a flame that emits ſtore of fuliginous ſteams, as in this tryal; but to that which is made of ſo pure a fuel as Spirit of Wine, that affords not ſuch ſteams (as in the former experiment;) *Whether*, I ſay, this ſurvival proceed from this, That the Common flame and the Vital flame are maintained by diſtinct ſubſtances or parts of the Air; *or* that common Flame making a great waſte of the Aereal ſubſtance, they both need to

keep them alive, cannot so easily as the other find matter to prey upon, and so expires, whilst there yet remains enough to keep alive the more temperate Vital flame; *or* that both these causes, and perhaps some other, concurr to the *Phænomenon*, I leave to be consider'd.

EXPER. III.

Of what happen'd to the Light of Glo-worms in the Exhausted Receiver.

FOr the sake of those Learned men, that have thought the Light of Glo-worms and other shining insects to be a kind of effulsion of the *Biolychnium*, or vital Flame, that nature has made more Luminous in these little *Animals* than in others; and which

which a very eminent Phyſician of the Colledge of *London* affirms to have felt in a warm climate more than ſenſibly hot; I ſhall ſubjoyn on this occaſion ſome tryals made on Glo-worms, which elſe ſhould be referr'd to thoſe Experiments of mine about the Relation betwixt Air and Light, that you were formerly pleaſed to publiſh.

We took two *Glo-worms*, that ſhone vividly enough, eſpecially one of them, whoſe Light appear'd ſtrong and tincted as if it had been tranſmitted through a blew Glaſs: Theſe we laid upon a little plate, which we included in a ſmall Receiver of finer glaſs than ordinary, that we might the better ſee what would happen: And having for the ſame purpoſe removed the Candles, that no other Light might obſcure that of the Inſects, we waited in the dark till that

was conſpicuous, and then order'd the Air to be begun to be pump'd out; and, as we expected, upon the very firſt exſuction there began to be a very manifeſt diminution of the Light, which grew dimmer and dimmer, as the Air was more and more withdrawn, till at length it quite diſappeared, though there were young Eyes among the aſſiſtants. This darkneſs having been ſuffer'd to continue a long while in the Receiver, we let in the Air again, whoſe preſence (as we look'd for) reſtored at leaſt as much Light as its abſence had depriv'd us of. This experiment was repeated with one more of thoſe inſects; and the event was, that they all three gradually loſt their Light by the Exhauſtion of the Receiver, and regain'd it (with ſome increaſe, as was judged) by the return of the Air. And in this Experiment we let in

in the Air by degrees, and with an interval or two, to obſerve, as we did, that, as the diminution of Light was greater and greater when the Air was more and more withdrawn, ſo the returning ſplendor was gradually increas'd as we pleas'd to let in more and more Air upon the worms.

EXPER. IV.

Containing a variation and improvement of the foregoing Tryal.

BUt here I foreſaw, it might be ſuſpected, that the diſappearing of the Light in our Exhauſted Receiver did not ſo much proceed from any real, though but temporary, extinction or eclipſe of it, as from this, that the Glo-worms having, as I have often

often obſerved, a power of drawing the luminous part into the opacous part of their body, they might, finding themſelves prejudic'd by the withdrawing of the Air, hide their Light from our Eyes, without loſing it, till being again refreſhed by the return of the Air, they might be invited to protrude it again into the tranſparent part of their tails. This ſcruple ſeeming grounded upon the nature of the thing, I thought it worth while to remove it by the help of another obſervation, that I long ſince made and have mentioned elſewhere about Glo-worms. Which is this, that, if they be kill'd whil't they are ſhining, their luminous matter may continue to ſhine for a good while after 'tis taken out of their bodies; and accordingly having put ſome of that, we took out of the forementioned inſects, upon a little paper, and included

cluded it in the Receiver we imployed, the Candles being removed, we perceiv'd it to ſhine vividly enough before the Pump was ſet on work, and afterwards to grow dimmer and dimmer, as the Air was more and more drawn out, till at length it quite vaniſhed; and it re-appear'd immediately upon the Air's return. This experiment was reiterated twice more with the ſame ſucceſs for the main. But we took notice, that the luminous matter, after the Air was let in, ſeem'd to us not only to have regain'd its former degree of Light, but ſenſibly increas'd it, (as it once happen'd alſo in the Experiment made on the living worms;) which whether it was caus'd by any real change made by the receſs and acceſs of the Air in the matter it ſelf, or by the greater accuſtomance of our Eyes to the darkneſs of the place, I diſpute not; and ſhall only

only add this *Phænomenon* of one of our tryals, that having a mind to ſee, whether a very little proportion of returning Air would not ſuffice to reſtore ſome little Light to the diſappearing matter; it was ſomewhat ſtrange to obſerve, that ſo very ſmall a quantity of Air, as was let in before the Light was revived, was enough to make it become plainly viſible though but dimm: In which ſtate it continued, till we thought fit to let in more Air upon it. (Farther tryals I could not make with theſe Glo-worms, having receiv'd them but that night out of the Country, and being the next morning to begin a journey.)

EXPER. V.

EXPER. V.

Wherein the former Inquiry is farther proſecuted.

AFter the lately mentioned Tryals we made with the Gloworms, having procured two or three other of thoſe inſects, whereof one was judged to be as large as three ordinary ones, we found, when we had brought them out of the Country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a conſiderable degree of luminouſneſs in the under part of his tail, we put him into the ſmall Receiver formerly mention'd, to try, whether, after the death of the *animal*, the ſhining matter would retain its former properties; but at the firſt time the Air was pump'd

pump'd out after the uſual manner, the light was not only not aboliſhed, but continued vivid enough, and ſo it did, when the Air being let in and again withdrawn, the tryal was made a ſecond time. But being unwilling to abandon the Experiment till we tryed it yet further, I caus'd the Receiver to be exhauſted yet once or twice more, and at length I perceived, that the Light began to diminiſh, as the Air was withdrawn; and laſt of all it ſo diſppear'd that the by-ſtanders could not ſee it, whereas upon the readmiſſion of the Air the Light ſhone vividly as before, if not more bright. This Experiment was reiterated with the like ſucceſs, and in both theſe times the like happened to the Light of the dead one and of a living one that we included with it, to be able to compare them together; though there were this diſparity betwixt

twixt them, that the luminous part of the dead worm was much larger than that of the living, and the Light of the later appear'd of a very greeniſh blew, whereas that of the former ſeem'd to be of a white yellow.

EXPER. VI.

Made to examine whether Animals be heavier dead than alive.

'TIs a receiv'd Tradition, that bodies when dead are much heavier than the ſame were when alive; the matter of fact being taken for granted, ſome will perhaps aſcribe the change to the utter inability of a dead body any way to aſſiſt thoſe that endeavour to remove it. But, according to the general opinion, this difference proceeds from the total extinction or receſs

receſs of the ſpirits vital and animal, which being ſuppos'd to be not only agil but light, leſſen'd the weight of the body they enliven'd; and Flame being conceiv'd to be the lighteſt among bodies here below, 'tis not improbable that ſome will aſcribe the *Phænomenon* to the levity of the Flame, which by being diffus'd through the body of an Animal, and vivifying it, deſerves the name of Vital. But I would not adviſe any to rely on this conceit, till they are duly ſatisfied of the truth of the matter of fact, which becauſe I have not yet found that any has endeavour'd to try, I ſhall on this occaſion give you the following tranſcript of one of my Notes about Statical Experiments.

A *Mouſe* weighing about three drachms and a half, being put in one of the ſcales of a very nice ballance,

was

was counterpois'd together with a ſtring that was tied about his neck like a nooſe, and after a while by drawing the ends of it was there ſtrangled. As ſoon as we judg'd him quite dead, we weigh'd him again, and though nothing was ſeen to fall from him; yet, contrary to the receiv'd Tradition, that Bodies are much heavier dead than alive, we found the weight to have loſt about $\frac{7}{16}$ of a grain; which probably proceeded from the avolation of divers ſubtil particles upon his violent and convulſive ſtruglings with death. But this was no more than an Experiment of this kind, made ſome years ago, induc'd me to expect and foretell.

Afterwards in a larger Ballance, but a very good one, purpoſely made for nice Experiments, we took a very young Catlin, of between 10. and 11. ounces in weight, and caus'd him to

be ſtrangled on the ſame ſcale, wherein he had been put. But he could not be diſpatch'd ſo ſoon as an ordinary full grown animal; ſo that by that time he was quite dead, we found him not only not to be grown heavier but lighter by four grains; which did not much ſurprize us, having elſewhere noted the life of ſo very young Creatures of that kind not to be eaſily deſtroy'd for want of Reſpiration. And I remember, that, for tryals ſake, another Catlin of the ſame Litter with this I have mention'd, being included in a Receiver, wherein another Animal of that ſize might probably have been diſpatch'd in two or three minutes, by the pumping out of Air, was kept there ſomewhat above a quarter of an hour before he appear'd to be quite dead.

NEW

ADVERTISEMENT.

These two following Attempts falling into the hands of the Author after the preceding Experiments were printed, it was thought fit to annex them here for the affinity of the subject.

Place this after Page 130.

An ATTEMPT

To produce Living Creatures in Vacuo Boyliano.

IN reference to the Opinion of thoſe Naturaliſts, that hold the *Seeds* of Living Creatures to be animated, and eſpecially to the *Hypotheſis* of thoſe Learned men that aſſert the *Flamma Vitalis* lately mention'd; it may be an inquiry of moment, Whether or no in the Seminal Principles, or Rudiments of Animals, the manifeſt operations of Life may be excited without the concurrence of the *Air*, whoſe intereſt in the production and conſervation of *Flame* may be gather'd from the foregoing Experiments. For, it ſeems likely to prove no inconſiderable diſcovery in reference to the lately men-

tion'd *Hypotheſis*, if it be found, that the *Principle* of *Life* in Seminal rudiments needs, as well as other *Flames*, the concurrence of the Air to actuate it.

I thought fit therefore, notwithſtanding the great and almoſt inſuperable difficulties, which 'twas eaſie enough for me to foreſee I ſhould meet with, to attempt the hatching of Eggs in our *Vacuum*; but though I made ſome unſucceſsful tryals of this kind in order to a diſcovery about Reſpiration, (not here to ſpeak of the attempts I made about the animation of *putrid* matter,) yet leaving the mention of them to its proper place, I ſhall only take notice in this what directly concerns the preſent inquiry. Conſidering then that pregnant females cannot be made to live and bring forth young in our exhauſted Receiver, and that the Eggs of Birds

and

and ſuch greater animals do in this colder climate of ours require to be hatched by the incubation of the females (or other Birds;) I thought the fitteſt ſubjects, I could both make choice of and procure for the deſign'd Experiments, would be the *Eggs* of *Silk-worms*: For, having many years ſince tried ſeveral things about thoſe Inſects, and among others found, that their Eggs would be hatched, not only by the heat of ones body, though that be the uſual way, but by the warmth of the Sun even here in *England*, if they be kept till the Spring be far enough advanc'd: Remembring this (I ſay) I got a good number of Silk-worms Eggs; and having caus'd three conveniently ſhap'd, but very ſmall, Receivers, to be purpoſely made, that differ'd very little (and that accidentally) either in ſize or figure, we conveyed into each

of them, together with a ſmall ſtock of Mulberry-leaves, ſuch a number of Eggs as we thought ſufficient to make one morally ſecure, that at leaſt ſome of them were prolifick; this done, we carefully exhauſted one of them, and ſecur'd it againſt the return of the Air; the two others we left full of Air: But having left in one, a little hole for the Air to come in and get out at, we ſtop'd the other ſo cloſe, as to hinder all intercourſe between the included Air and the External. All things being thus præpared, we expos'd the Receivers to a South-window, where they might lie quiet, and where I either came, or ſent to look on them from time to time; the ſpring being then ſo far advanc'd, that I ſuppos'd the heat of the Sun would be of it ſelf ſufficient to hatch them in no long time.

As

As to the ſucceſs of this tryal, my not being able to find any regiſter of the particular *Phænomena* that occurr'd, keeps me from venturing to relate it very circumſtantially; but this I remember in general, that both I and others took notice, that in the unexhauſted Receivers there were divers Eggs hatch'd into little Inſects that perforated their ſhells, and crept out of them; though afterwards for want of change of Food, or Air, or both, few or none of them proved long-liv'd. But though the Eggs in *theſe* Receivers began to afford us little animals in a few dayes; yet the Eggs in the *exhauſted* Receiver did not, in many more, afford us any. And though I will not venture to ſay how long preciſely we kept them in the ſame window, after ſome of the above-mention'd Eggs were hatched; yet (if I much miſtake not) 'twas

(from first to last) about three or four times as long; and I remember, we kept them till it was thought to no purpose to wait any longer, and agreed in imputing the not hatching of the Eggs by the so long continued action of the Sun to the absence of the Air.

What other *Phænomena* occurr'd to us in making this Experiment, and another not unprosperous one upon the Eggs of Flyes, you may expect, when I can light on my Notes about them, or have my memory refresh'd by those that assisted at the making of them.

EXPER. VII.

An ATTEMPT

Made upon Gnats *in our* Vacuum.

I Elſewhere mention that it has been obſerv'd, by a couple of our *Virtuoſi* (whom I there name,) and ſeveral times by Me, that (here in *England*) multitudes of Gnats are generated of little animals that live, for a part of the Summer, like Fiſhes in the water; and conſidering, that by theſe a very unuſual paſſage is made from *Swimming* to *Flying* animals, I thought them very fit ſubjects, whereon to make the following Experiment.

[*Partly* to try whether at leaſt an *animal* already living and moving in our *Vacuum* may be able to attain the perfection due to it according to the

the courſe of Nature; and *partly* to examine, whether, in caſe he ſhould attain it, at leaſt the lighter ſort of winged Inſects may be able to fly in that place; and *partly* to diſcover, whether an animal, that had long liv'd in our *Vacuum*, would, when turn'd to a Flie, be able to continue alive without a Reſpiration, he had never been accuſtom'd to, in its priſtine form or ſtate; we took divers of thoſe little ſwimming Creatures, which, in Autumn, eſpecially towards the end of it, are wont to be turn'd into Gnats, and having put a convenient number of them together in a fit quantity of Rain-water, wherein they had been found and kept, into a ſmall Receiver, the Air was pumpt out, and the veſſel ſecur'd againſt its return, and then ſet aſide in a place, where I could obſerve, that the day after ſome of theſe little animals were yet alive and

ſwim-

ſwimming to and fro, not without minute bubles adhering to them; but at the end of a day or two after that, I could not perceive any of them to ſurvive their dead Companions, nor did any of them recover, when freſh Air was let in upon them. But though this Experiment were the beſt I was then able to make, yet I reſolv'd, if God ſhould vouchſafe me life and health, to repeat it the enſuing Autumn; that, wherein it was made, proving ſo cold and unſeaſonable, that a number of theſe little Creatures, put up with water into another ſmall Receiver, died all within a few days, though none of the Air was exhauſted; and ſeveral, that I kept in an ordinary Glaſs, that was divers times unſtop'd to give them freſh air, did yet periſh at no ordinary rate. And I confeſs (as unkind as this trouble of mine may ſeem to the

the Air;) that the failing of this and ſome other Experiments of producing Animals in our exhauſted Receivers was the more unwelcome to me, becauſe I had and have ſtill a great deſire to ſee, if it be poſſible, what would happen to Animals, which had been produc'd in a place free from the preſſure of the Atmoſphere, as if they had been born in *Epicurus's* imaginary *intermundane* ſpaces, upon their coming to be ſuddenly ſurrounded with our heavy Air, and having their tenderly fram'd bodies expos'd to its immediate preſſure.

NEW

NEW EXPERIMENTS ABOUT Explosions.

(Annex'd by way of Appendix to the former Papers.)

NEW EXPERIMENTS ABOUT EXPLOSIONS.

(Annex'd by way of Appendix to the former Papers.)

FOr as much as ſome of the Learned men, that are the grand Aſſertors of the *Flamma Vitalis* (whoſe opinion occaſion'd my preſenting you the foregoing Experiments,) do alſo with the juſtly famous *Doctor Willis* explicate many of the motions of Animals, eſpecially thoſe performed in the Muſcles, by the *Exploſions* made of certain juices or fluid ſubſtances of the Body, when they come to mingle with each other: And for as

as much alſo as I do not remember, I have heard the Maintainers of this *Hypotheſis* inſiſt on other inſtances in favour of it, than the going off of Gunpowder; which being not a liquor but a conſiſtent and brittle body, and requiring for its exploſion either Actual Fire, or a far intenſer heat than can be ſuppoſed natural in Men and other Animals; I was induc'd to ſuſpect, they were not yet provided with better Examples; and therefore I preſume, it will be look'd upon as a thing neither uſeleſs, nor altogether impertinent, if, without offering to determine any thing about the truth of the opinion, I ſupply the embracers of it with two or three examples of Exploſions made by the bare mingling of liquors; which I ſhall borrow from the elſe-where mention'd *Notes*, that I drew up ſome years ago, in order to the improve-

improvement of ſome parts of *Phyſick.*

EXPER. I.

Of an Exploſion made with the Spirits *of* Nitre *and* Wine.

WE took Spirit of Nitre, ſo ſtrong, that the fumes made the upper part of the Glaſs, it was kept in, always reddiſh, and having put but one ounce of it into a bolt-head with a long neck, capable to contain, as we gueſs'd, twelve or ſixteen times as much, we caus'd an equal weight of *Alkhool* (or highly rectified Spirit of Wine) to be taken, and a little of it being put to the Spirit of Nitre, it preſently made ſo ſtrong

and quick an expanſion or exploſion, that ſome of it flew out of the Glaſs and hit againſt the cieling of the room, (where I ſaw the mark of it,) and falling upon his face that held the Glaſs, made him think (as he told me) that fire had fallen upon it, and made him run down the ſtairs like a mad man to quench the heat at the Pump. Wherefore bidding the Laborant proceed more warily, I order'd him to put into the Bolt-head but part of a ſpoonful of Spirit of Wine at a time; and yet at each of a pretty many affuſions, that I tay'd to ſee the effect of, there would be a great noiſe, as of an ebullition, though no ſtore of froth produc'd, and accompanied with ſo great a heat that I could not hold the Glaſs in my hand; and immediately there would iſſue out a copious and red ſmoak; to which when I caus'd a little Candle

to

to be held, though at near half a foot distance from the top of the bolt-head, it would presently take fire, and burn at the top of the bolt-head like a flame at the upper end of a Candle, till I caus'd it to be blown out, that fresh Spirit of Wine might be pour'd in; which when it was all mingled with the other liquor, the heat and conflict caused.

Divers other *Phænomena* relating to this Experiment (by which I intended to make out more things than one,) belong not to our present subject, and are already set down in other Papers. But yet 'twill be pertinent to shew in this place, that the noise and ebullition produc'd in this mixture is not unaccompanied with a briskly Expansive or an Explosive motion. To make then an Experiment to this purpose, and yet avoid the danger whereto the making of it

unwarily might expoſe both the veſſels and us; we put an ounce of ſuch ſtrong Spirit of Nitre, as is above mentioned, into a moderately large bolt-head furniſh'd with a proportionable ſtem, over the orifice of which we ſtrongly tied the neck of a thin Bladder, out of which moſt part of the Air had been expreſs'd, and into which we had conveigh'd a ſmall Viol, with a little highly rectified Spirit of Wine: Then this Viol, that before was clos'd with a cork, being unſtopp'd without untying or taking off the Bladder, a ſmall quantity, by gueſs not a quarter of a ſpoonful, of the Alcohol of Wine was made to run down into the Spirit of Nitre, where it preſently produc'd a great heat and commotion, and blew up the Bladder as far as it would well ſtretch, filling alſo the ſtem and cavity of the Glaſs with very red fumes,

fumes, which preſently after forced their way into the open Air, in which they continued for a good while to aſcend in the form of an Orange-colour'd ſmoak.

EXPER. II.

Of an Exploſion made with Oyl *of* Vitriol *and* Oyl *of* Turpentine.

IF I had at hand the Papers you have divers times heard me ſpeak of about *Heat*, I could give you the particulars of ſome Tryals about Exploſion, that perhaps you would think more pertinent than deſpicable; but for want of thoſe Papers I muſt content my ſelf to tell you in general: That I remember, that I have more than once taken ſtrong *Oyl* of *Vitriol* and common *Oyl* of *Turpentine*, and warily mix'd them in a certain proportion by ſhaking them very well together; and that thereupon inſued (what I had reaſon to look for) ſo furious an agitation

tation of the minute parts of the mixture, and ſo vehement or ſudden Expanſion or Exploſion, as did not only ſeem ſtrange to the Spectators, but would have prov'd dangerous too, if I had not taken care before-hand, that the Tryals ſhould be made in a place where there was room enough, and that even the *Operator*, that ſhook the veſſel, ſhould ſtand at a convenient diſtance from the mixture.

EXPER. III.

About an Explosion made by two Bodies actually Cold.

I Remember not, that I found the Assertors of Explosions in Animals to have taken notice of a difficulty, which to me seems not unease to be observ'd, and yet very worthy to be cleared. For 'tis known, that Fishes, and those especially of the vaster sort, can move and act in the waters with a stupendious force; and yet it is affirm'd by those that pretend to know it, that the Blood of most Fishes is still actually cold: And

And I remember, I found the blood even of thoſe I diſſected alive, to be ſo. From whence moſt men would argue, that even in the vaſt Sea-monſters, there can be made no Exploſions, theſe being ſtill effected by or accompanied with an intenſe degree of heat.

'Twere incongruous to my deſign, to examine this difficulty as it directly regards the Exploſions, ſaid to be made in Animals: But ſpeaking of Exploſions in general, perhaps I might do the favourers of *vital* ones (if I may ſo term them) no unacceptable piece of ſervice, by experimentally ſhewing, that 'tis not impoſſible, though it ſeem very unlikely, that Exploſions ſhould be made upon the mixture of bodies, which, whilſt they ſeem to put one another into a ſtate of Effervеſcence, are

are really cold, nay colder than before their being mingled. Of these odd kind of mixtures, I remember I have in another * paper set down some Tryals that I made to other purposes, as well with two liquors as with a liquor and a solid body; which later sort I there mention my having made by an improvement of an experiment of the excellent *Florentine Virtuosi*. And among those Tryals I find one, whose pertinency to the matter in hand invites me to annex as much of it as is proper in this place.

* About the Production or Extrication of Air.

There were put two ounces of powder'd *Sal Armoniac* into a pretty large Glass-tube Hermetically seal'd at one end; into the same a slender Glass-pipe, furnished with two ounces of *Oyl* of *Vitriol*, was so

ſo put, that, when we pleas'd, we could make the liquor run out into the larger Tube, which, after theſe things were done, was clos'd exactly, ſo that nothing might get in or out. My deſign was, that this inſtrument ſhould be ſo warily inverted, that the *Operator* might get out of the way, and the Oyl of Vitriol, falling ſlowly upon the Sal Armoniac, ſhould, without producing any heat, produce an exploſion not dangerous to the By-ſtanders. But whilſt I was withdrawn to a neighbouring place to write a Letter, the Operator not ſtaying for particular directions, raſhly inverted the inſtrument without taking care to get away: whence it happen'd, that as ſoon as ever the contained liquor, being too plentifully poured out, came to work on the Sal Armo-

niac,

niac, wherewith it is wont to produce cold, there was ſo ſurprizing and vehement an Expanſion or Exploſion made, that with a great noiſe, (which, as the Laborant affirmed, much exceeded the report of a Piſtol,) the Glaſſes were broken into a multitude of pieces, many of which I ſaw preſently after, and a pretty deal of the mixture was thrown up with violence againſt the Operators Doublet and his Hat, which it ſtruck off, and his face; eſpecially about his eyes, where immediately were produc'd extreamly painful tumors, which might alſo have been very dangerous, had I not come timely in, and (to add that upon the by) made him forthwith diſſolve ſome *Saccharum Saturni* in fair water, and with a ſoft ſpunge keep it conſtantly moiſten'd by

by very frequently renewed applications of the Liquor: By Gods bleſſing upon which means, within an hour or two, the pain, that had been ſo raging, was taken away, and the fretting Oyl of Vitriol was kept from ſo much as breaking the skin of the Tumors that it had made.

The firſt part of the Relation of this tryal might have been omitted, or at leaſt ſhorten'd, unleſs I had deſign'd to communicate unto you a way of doing what I do not know to have been attempted by others, namely to put bodies together when and by what degrees one pleaſes, after the Glaſs that contains them has been Hermetically ſeal'd up; which Mechanical contrivance, eſpecially as it may be varied, may be, as I have try'd, uſefully

fully apply'd to more purpoſes than 'twere proper here to take notice of.

But to conclude with a word or two touching the foregoing Experiment; I ſhall only add, That another time we made a like tryal a ſafer way, by tying a Bladder ſo to the top of a Bolt-head, into which we had before-hand put the *Sal Armoniac*, that, by warily moving the Bladder, whence the Air had been expreſs'd, we could make ſome of the *Sal Armoniac*, we had lodg'd in its folds, to fall upon the liquor, with which it preſently made an Exploſive mixture, that quickly blew up the Bladder.

But theſe, Sir, *are bare Conjectures, left to be, after a farther diſcuſſion, (if you think them worthy*

thy of it) determin'd by You, to whom as theſe Papers are addreſs'd, ſo they are alſo ſubmitted by the Writer of them,

Who is

Sir

Your moſt, *&c.*

NEW

A N

HYDROSTATICAL Diſcourſe

Occaſioned by

The Objections of the Learned Dr. *Henry More*,

AGAINST

Some Explications of New Experiments made by Mr. *Boyle*;

AND

Now publiſh'd by way of PREFACE to the Three enſuing Tracts.

ADVERTISEMENT.

THis Hydroſtatical Diſcourſe (diſtinguiſhed by ſmall letters for the Signature) is to be placed immediately before the Title, *New Experiments of the Poſitive and Relative Levity of Bodies under Water.*

TO The Reader.

WHen I determin'd to write this Polemical Discourse, I did not forget, that when I first ventur'd some of my trifles abroad into the world, my friends obtain'd from me a promise that, after I should have answer'd the two first that should expresly write against me (which happen'd to be the Learned Linus *and Mr.* Hobbs,) *to shew that I was not altogether unacquainted with a way of defending Truths, I would afterwards write no Book in answer to any, that should come forth against mine; for not only my friends, but I, thought it enough for a person that never was a Gown-man, to communicate freely*

his thoughts and Experiments to the Curious, without despairing, that those things, that should be evidently true, would be able to make their own way, and such as were very probable would meet with Patrons and Defenders, in so inquisitive an age as ours. And indeed I do not find, that either upon the account of my Writings, or Ingenious mens opinion of them, I have had much cause to repent the keeping of my promise, notwithstanding the Writings, that have impugn'd some of mine, but without much prejudice, that I know of, either to the propos'd Truths, or the Proposer of them. And therefore I should not at all have enter'd upon a defence of what is attaqued of mine by the Learned Doctor More, *if I had not suppos'd, that it would not require a* Book, *but might be dispatch'd in a* Preface: *For, having by me some little Tracts, that should, though the Doctor had*

had never engag'd me, have been imparted to the Publick, and observing that the new Experiments contain'd in one or other of them, would by an easie application be brought to confirm my formerly deliver'd explications of other Phænomena, *and enervate the Doctors Objections against them, I thought I might without long troubling the Reader, or my self, defend what I look'd upon as Truth, by answering some incidental passages of the Doctors discourse, and referring the Reader, for the main points in Controversie between us, to those Experiments of the following Tracts, which clearly contain the grounds of deciding them. But yet this Consideration would not perhaps have engaged me to write the following Preface, if the Objections I was to answer had not been, by a Person of so much Fame, propos'd, with so much confidence; and though with*

very great Civility to me, yet with such endeavours to make my Opinions appear not only untrue, but irrational and absurd, that I fear'd his discourse, if unanswer'd, might pass for unanswerable, especially among those Learned men, who, not being vers'd in Hydrostaticks, would be apt to take his Authority and his Confidence for cogent Arguments; and who (not observing how liberal some men are of titles to the Arguments that please them) would make a scruple of thinking, that what is with great solemnity deliver'd for a Demonstration in a Book of Metaphysicks, can be other than a Metaphysical Demonstration. The Care therefore, that what I judge to be true, should not be made to pass for absurd, which is a degree beyond what is meerly Erroneous, by being so severely handled by a person of Doctor More's *fame and Learning, induc'd me to begin the following*

lowing Paper, which should have been shorter than now it is, but that I was perswaded to lengthen it beyond what was either necessary or design'd, that I might, by the addition of some few thoughts and Experiments on the occasions that were suggested to me, endeavour to clear up and confirm some Hydrostatical Truths, that, I fear, are but by very few either assented to, or perhaps so much as understood, and so might make the Reader amends for the trouble I was forc'd to give him in a Dispute which I apprehended he might otherwise think himself but little concerned in. And he will, I hope, easily discern, that I have no mind to burthen him in my Preface with things not pertinent to the scope of it, if he take notice, that both for his sake and the Learned Doctors, (whose Civility I would not leave unanswered) I have restrain'd my self to the Defensive

part, forbearing to attaque any thing in his Enchiridium Metaphyſicum, *ſave the two Chapters wherein I was particularly invaded.*

But though I have declin'd the delivering my Opinion of the Doctors Book; yet I dare not forbear owning my not being ſatisfied with that part of his Preface, which falls foul upon Monſieur des Cartes *and his Philoſophy. For though I have often wiſh'd, that Learned Gentleman had aſcrib'd to the Divine Author of Nature a more particular and immediate efficiency and guidance in contriving the parts of the Univerſal Matter into that great Engine we call the* World; *and though I am ſtill of Opinion, that he might have aſcrib'd more than he has to the Supreme Cauſe in the firſt Origine and Production of things Corporeal, without the leaſt injury to Truth, and without much, if any, prejudice*

judice to his own Philoſophy; and though not confining my ſelf to any Sect, I do not profeſs my ſelf to be of the Carteſian: yet I cannot but have too much value for ſo great a wit as the founder of it, and too good an opinion of his ſincerity in aſſerting the exiſtence of a Deity, to approve ſo ſevere a Cenſure as the Doctor is pleaſed to give of him. For I have long thought, that in Tenets about Religion, though it be very juſt to charge the ill conſequences of mens opinions upon the Opinions *themſelves; yet 'tis not juſt, or at leaſt not charitable, to charge ſuch Conſequences upon the* Perſons, *if we have no pregnant cauſe to think, they diſcern them, though they diſclaim them. And ſince men have uſually the fondneſs of Fathers for the Off-ſpring of their own Brains, I ſee not, why* Carteſius *himſelf may not have over-look'd the bad inferences, that may be drawn*

from

from his Principles, (if indeed they afford any such,) since divers Learned and not a few pious persons, and profess'd Divines of differing Churches, have so little perceiv'd, that the things objected are consequent to such Principles, that they not only absolve them as harmless, but extol them as friendly and advantageous to natural Religion. And I see not, why so great and radiant a Truth, as that of the Existence of a God, *that has been acknowledg'd by so many meer Philosophers, might not as well impress it self on so capable an intellect as that of Monsieur* des Cartes, *or that so piercing a wit may not really believe he had found out new* Mediums *to demonstrate it by. And since the Learned* Gassendus, *though an Ecclesiastick, had been able as well safely as largely to publish the irreligious Philosophy of* Epicurus *himself; it seems not likely, that so dextrous a*

wit

wit as that of Monſieur des Cartes, could not have propos'd his notions about the Mechanical Philoſophy, without taking ſo mean a courſe to ſhelter himſelf from danger, as in the moſt important points that can fall under mans conſideration to labour with great skill and induſtry to deceive abundance of ingenious men, many of which appeared to be Lovers of Truth, and divers of them Lovers of Him alſo. And I am the more averſe from ſo harſh an opinion of a Gentleman, whoſe way of writing, even in his private Letters, tempts me very little to it, becauſe I cannot think him an Atheiſt and an Hypocrite, without thinking him (what Doctor More has too much celebrated him) to call him a weak head, and almoſt as bad a Philoſopher as a Man. For as far as I underſtand his Principles, ſome of the moſt important points of his Philoſophy (which if it were

needful

needful I could name) *are interwoven with the Truth of the Existence of a God, or do at least suppose it, and are not demonstrable without it. But I must not prevent the* Cartesians, *who, now he cannot do it for himself, I doubt not will Apologize for their Master; though looking upon him as a great Benefactor to, though not the first founder of the Mechanical Philosophy, I could not consent by a total silence, upon such an occasion, to become any way accessary to the blemishing of his Memory.*

A N

AN HYDROSTATICAL DISCOURSE

Occasion'd by the Objections of the Learned Dr. Henry More, *against some Explications of New Experiments made by Mr.* Boyle; *and now publish'd by way of* PREFACE *to the Three ensuing Tracts.*

SIR,

UPon the Advertisement you gave me yesternight, that I was particularly concern'd in the Learned Doctor *More's Enchiridium Metaphysicum*, I this day turn'd over the leaves of one, which I have freshly receiv'd from the Reverend Author himself: And being assisted by the *series* of the Titles, I quickly lighted on that part of

of the Book, whoſe ſubject made me expect to find my ſelf queſtion'd there, as I preſently found I was. For though that civil Adverſary is pleas'd to omit my name, and, the farther to diſguiſe it, imploys inſtead of it a great and unmerited *Encomium*; yet by the Book he cites, and the Experiments againſt which he argues, 'tis very eaſily diſcoverable, that his objections are meant againſt me, who ſee yet no cauſe at all to be ſcrupulous to own my Name, and the Doctrine deliver'd in the paſſages he is pleas'd to oppoſe.

I doubt not but you will preſently deſire to know, what I think of this much expected work; but when I have told you, that I have gained time to peruſe only (and that but curſorily) the 12*th*. and 13*th*. Chapters, you will, I queſtion not, excuſe a perſon that does exceedingly want health,

health, and yet wants not almoſt continual avocations; if I now content my ſelf to give you my thoughts of that part of the newly mention'd Chapters, which properly relates to me; I ſay, that part of the Chapters, becauſe there are others wherein I need not intereſs my ſelf. For to omit other Paragraphs, the Doctor has in the former part of the 13th Chapter thought fit to ſeparate from my explication of the *Phænomena* in queſtion betwixt us, that of the Learned *Henricus Regius*; and the later part of the ſame Chapter he imploys in an ingenious diſpute againſt thoſe that would have the Aerial Particles act with perception and deſign, and (as he ſpeaks) *pro re nata*, which Opinion you will eaſily believe I neither was of, nor am like to adopt. S. 8. 16, 19.

It

It remains then, that ſetting aſide thoſe diſcourſes of the *12th* Chapter, wherein it is needleſs that I ſhould make my ſelf a party; I proceed to conſider thoſe Paragraphs, which will be eaſily gueſs'd to be levell'd at my Explications, and by which I muſt confeſs, I cannot at all be yet convinc'd of their being falſe ones. But in doing this, I ſhall not only in complyance with my preſent haſte, but alſo to expreſs my reſpect to the Learned Doctor, forbear to ſay any more, than what I ſhall judge requiſite to anſwer the Objections, that directly concern my own Explications, without medling, by way of Retaliation, with his *Hypotheſes* or Opinions, or endeavouring to ſet any paſſages of his Writings at variance among themſelves, or to take thoſe little advantages, which are uſually ſought for by Diſputants.

I ſhall not trouble you, nor tire my ſelf with any Schemes, ſince the Doctor has taken the pains to inſert thoſe that are neceſſary for his purpoſe in his Book, and I have not my own at hand. Wherefore, not doubting that you have by you thoſe Books of mine he refers to, and ſuppoſing that you will, whilſt you are reading, have alſo his Book with the inſerted Schemes before your eyes, I ſhall not ſpend time on any further Preamble, but immediately enter upon the conſideration of the Objections I am to anſwer.

THE FIRST SECTION.

CHAP. I.

THe firſt Explication of mine, that the Learned Doctor animadverts upon in his 12*th* Chapter, is, that which I give in the 33*th* of my Phyſico-Mechanical Experiments, touching the Spring and Weight of the Air; where I relate, that the Sucker in the Air-Pump of our Engine, having been forcibly depreſs'd to the lower part of the Braſs *Cylinder*, which yet was carefully cloſed at the top, ſo that the cavity of the Cylinder was empty of Air; this Sucker, I ſay, would in this caſe appear ſpontaneouſly to remount towards the top of the Cylinder, though it

it were clogg'd with a hundred pound weight to hinder its aſcent. Which *Phænomenon* I aſcrib'd to this, that the Sucker being, by the withdrawing of the Air in the Cylinder, freed from the wonted force of the ſpringy Air that endeavour'd to depreſs the internal part of it, was not inabled by the appendant weight to reſiſt the preſſure of an Atmoſpherical Cylinder equal in diameter to it, which, preſſing againſt its lower or external ſurface, endeavour'd to impel it up.

Now the Doctor having in the two firſt Paragraphs made a Deſcription of my Engine, (which I ſhall now paſs over) does in the third teach us, that the Corporeal cauſe, if there be any, of the aſcent of the Sucker, muſt be, either in the Sucker it ſelf, or in the almoſt exhauſted cavity of the Cylinder, or laſtly in the exter-

nal Air : Which premiſed, he does in the ſame third Section, and in the fourth, endeavour to prove at large, that the cauſe is to be derived neither from the one, nor from the other of the two firſt. And therefore I, that maintain neither of the Opinions he diſputes againſt, ſhall leave thoſe Paragraphs of his untouch'd. Nor ſhall I meddle with the fifth, ſixth, and ſeventh, where he argues againſt the explications of ſome, that would ſolve the *Phænomenon* upon ſome *Carteſian* grounds, and as well amply as particularly againſt the ſolution that he ſuppoſes would be given of it congruouſly to his own Sentiments by the Learned *Regius*. Theſe Diſcourſes, I ſay, of the Doctors I leave untouch'd ; becauſe 'tis at length in the eighth Paragraph, that he impugnes that ſolution of the *Phænomenon*, which he aſcribes to me, whoſe Opinion

Opinion he firſt delivers, though not juſt in the terms I would expreſs it my ſelf; yet I dare ſay very ſincerely, and ſo near my ſence, that I ſhall forthwith paſs from the eighth Section to the beginning of the ninth, where he begins to propoſe his Objections, which he is pleas'd to uſher in with a complement to me, that I ſhould be very vain if I looked upon as any thing more than a Complement.

To his firſt Objection, propos'd in theſe words, * *Primò enim, ſi hæc ſolutio verè mechanica ſit, quæ tandem Cauſa verè mechanica aſſignari poteſt gravitationis ſingularum particularum, totiúſque atmoſphæræ in ſuis locis? Nam quod materiam ſubtilem attinet*, &c. I anſwer, that I did not in that Book intend to write a whole Syſteme, or ſo much as the Elements of Natural Philoſophy; but

* 139.

having sufficiently proved, that the Air, we live in, is not devoid of weight, and is endowed with an Elastical Power or springiness, I endeavour'd by those two Principles to explain the *Phænomena* exhibited in our Engine, and particularly that now under debate, without recourse to a *Fuga Vacui*, or the *Anima Mundi*, or any such unphysical Principle. And since such kind of Explications have been of late generally called Mechanical, in respect of their being grounded upon the Laws of the Mechanicks; I, that do not use to contend about Names, suffer them quietly to be so: And to entitle my now examined Explication to be Mechanical, as far as I pretend, and in the usual sence of that expression, I am not obliged to treat of the cause of Gravity in general; since many Propositions of *Archimedes*, *Stevinus*, and

and thoſe others that have written of Staticks, are confeſſed to be Mathematically or Mechanically demonſtrated, though thoſe Authors do not take upon them to aſſign the true cauſe of Gravity, but take it for granted, as a thing univerſally acknowledged, that there is ſuch a quality in the Bodies they treat of. And if in each of the Scales of an ordinary and juſt Ballance, a pound weight, for inſtance, be put; he that ſhall ſay, that the Scales hang ſtill in *Æquilibrium*, becauſe the equal weights counterpoiſe one another: and in caſe an ounce be put into one of the Scales, and not into the oppoſite; he that ſhall ſay that the loaded Scale is depreſs'd, becauſe 'tis urged by a greater weight than the other, will be thought to have given a Mechanical Explication of the *Æquilibrium* of the Scales, and their loſing

it; though he cannot give a true cauſe, why either of thoſe Scales tends towards the Center of the Earth. Since then the aſſigning of the true cauſe of Gravity is not required in the Staticks themſelves, though one of the principal and moſt known of the Mechanical Diſciplines; Why may not other Propoſitions and Accounts, that *ſuppoſe* Gravity in the Air, (nay *prove* it, though not *à priori*) be look'd on as Mechanical?

CHAP. II.

THe next thing the Doctor oppoſes to my Explication, is a reſolute Denial, that there is any ſuch Gravitation, as I pretend, of Bodies, or their Particles, in their proper

proper places. But becauſe, for the proof of his negation, he refers us to the next Chapter, we ſhall hereafter have a fitter place than this to conſider it in.

Thirdly, he tells us, we may juſtly doubt of the equal diffuſion of the Springy power, or the Preſſure of the Air every way. In what ſence, in ſome caſes, I admit of a ſmall inæquality between the preſſure of Fluids againſt differing parts of a ſurrounded body, I have * elſewhere declared, and need not here diſcourſe of; ſince in the caſe before us, and in the like, that Preſſure is inconſiderable enough to be ſafely neglected. And whereas our Author thus argues, * *Semotâ vi Elaſticâ, particulæ tamen Atmoſphæræ deorſum tenderent. Eſt igitur depreſſio quædam deorſum præter vim Elaſticam ipſi*

* See the Hydroſtatical Paradoxes, eſpecially Parad. 7.

* p. 139.

ipſi ſuperaddita; ſurſum non item ſed elaſtica ſola, éſtque ſuppar ratio in preſſionibus tranſverſis & obliquis. I preſume, he did not ſufficiently conſider our Hypotheſis and the nature of the preſſure of Fluid Bodies that have weight: For Water, to which no Springineſs is aſcribed, as there is to Air, but which acts by its weight and fluidity, is able upon the ſcore of thoſe Qualities to buoy up great Ships, that the ebbing Tide often leaves upon the ſtrand.

And whereas the Learned Examiner propoſes a fourth Objection in theſe terms, * *Quibus omnibus addas, difficile eſſe intellectu, ſi unius Cylindri Atmoſphæræ pondus æqualis diametri cum Embolo reflectione in fundum Emboli derivetur, cur non quinque alii Cylindri Aeris qui circumſtant Embolum in ejus fundum eodem modo ſimul agere poſſunt, ita ut*

* p. 139.

vis

vis ſurſum impellens Embolum ſextuplo major ſit quàm hactenus ab hujus opinionis fautoribus exiſtimata eſt. Quod ſi ſit, tunc certè, ſiquo artificio fieri poſſit ut unius ſolius Cylindri actio in Embolum admitteretur, re iquorum quinque excluſa, & pari tamen facilitate Embolus aſcenderet, manifeſtum indicium eſſet, ne unum quidem Cylindrum Atmoſphæræ agere in fundum Emboli, ſed totam Hypotheſin, ingenioſam tantummodo eſſe fictionem. I preſume, Hydroſtaticians will think, this might have been ſpared. For they will tell him, that there can no more of a fluid preſs directly upward againſt the Cylindrical Orifice of a Body immers'd in that fluid, than a Cylinder of that fluid of the ſame diameter with the Orifice (the lateral preſſures bearing againſt the lateral parts of the Cylinder.) And therefore if you invert, for inſtance, a

Pipe

Pipe open at both ends, and filled to a certain height with Oyl, into common water; the Oyl that is kept up by the preſſure of the water upwards, will keep at the ſame height as to ſenſe, whether the Veſſel that contains the Water be broad or narrow, provided it be ſomewhat larger than the Orifice of the Pipe.

And now, to invalidate yet further the precedent Objections, made by the Doctor, I ſhall add, that it need not be thought incredible, that the Atmoſphere by its weight, or the Spring of the Air compreſs'd by that weight, ſhould be able to raiſe up fourſcore or a hundred pound, hanging at the Sucker: Since I have * manifeſted two or three years ago by a clear and cogent Experiment, that a little air included in a Bladder will by its meer Spring be able

* See Continuat. of New Exper. Phyſico-Mechan. Exp. 48. p. 165.

able to heave up a weight of a hundred Pound, and this without the help of any rarefaction by heat. By which Experiment may be alſo confirmed, what I deliver'd a while ſince about the endeavour of the Air, that is wont to be included in our braſs Cylinder, by expanding it ſelf to thruſt away the Sucker (which, in regard of the ſtructure of the Pump, it can do no otherwiſe than downwards,) with a depreſſing force, æquivalent to the preſſure upwards of the Atmoſphere againſt the external part of the ſame Sucker.

CHAP. III.

BUt I ſhall not inſiſt upon the foregoing Objections, becauſe the Learned Doctor himſelf tells us, that

that their attempts may ſeem to be but light skirmiſhes in compariſon of that which follows. Whereunto I ſhall therefore apply my attention.

This grand Objection our Learned Adverſary takes from the already often-mention'd aſcent of the Sucker clogg'd with a hundred pound weight, and recommends by this introduction. * *Etenim ex ipſis Phænomeni viſceribus robuſtiſſimum jam contra omnem mechanicam illius ſolutionem Argumentum eruo, & quod non ſolum contra vim aeris elaſticam ſuprà dicto modo explicatam militat, ſed etiam contra Carteſianum illum aeris conatum nixúmque, &c.* Which premis'd, the Argument it ſelf is thus propos'd: *Eſt enim* (ſays he) *juxta hujus experimenti Phænomenon, vis illa aeris elaſtica (nixúſque expanſorius) major multo quàm quæ*

* p. 140.

p. 140.

quæ fieri poteſt à rerum natura, quámque quotidianis illis Phænomenis congruit. Nam ſi nixus hic elaſticus tantam vim elaſticam haberet ut plus centum pondo plumbum ſurſum poſſit propellere, omnes profectò rerum terreſtrium compages tantâ violentiâ comprimerentur, ut nullæ, niſi quæ admodum firmiter compactæ ſint, tantæ compreſſioni reſiſtere poſſent, quin confringerentur, vel partium colliſione ita contererentur ut brevi tempore perirent, &c.

Though this Objection be ſpecious enough, yet it preſents me with no difficulty, that I was not well aware of; as I preſume you will eaſily perceive by what you will meet with in the following Papers, eſpecially that, which conſiſts of Experiments and Conſiderations about the differing *Preſſures* of *Solids*, Weights, and *Ambient Fluids*. The nature of which Preſſure

Preſſure and its æquality (as far as in our controverſie 'tis needful to be ſuppos'd) will, I hope, ſatisfie you of the invalidity of the propos'd objections; eſpecially ſince the Doctrine it impugns, namely the Weight and Preſſure of the Atmoſphere, is not a bare *Hypotheſis*, but a truth made out by divers Experiments, by which even profeſs'd Oppoſers of it have publickly acknowledg'd themſelves to be convinced.

CHAP. IV.

IN the next Paragraph (which is the 11*th.*) the Learned Doctor adds a further Objection, wherein he ſuppoſes, that there is laid upon a wooden Scale, of the ſame diameter with the above-mention'd Sucker, a lump of Butter of the ſame largeneſs with

with the Scale. Whence he argues, that if our Hypotheſis take place, the Butter muſt be preſs'd againſt by two Cylinders of Air, the one preſſing it upwards, the other downwards, and the preſſure of them both amounting to two hundred pounds. But, ſayes he, the Butter is not preſs'd at all, as appears by this, that no ſerous humour is ſqueez'd out of it towards the edges, not ſo much as in thoſe parts that lie parallel to the *Horizon*, whence the Concluſion ſeems eaſie to be deduc'd.

But in the 12*th* Paragraph, the Doctor himſelf propoſes a Solution, which he might eaſily foreſee I would employ to invalidate his Argument; Namely, that the Air preſſing, as well againſt the ſides of the Butter, as againſt the top and bottom, hinders the Maſs from horizontally extending it ſelf. And whereas, by

way of reply to this ſubterfuge, as 'tis call'd in the margent, he ſubjoynes, * *Cui reſpondeo, quòd tamen hoc nihil prohibet quo minùs in omnes partes horizontales exprimatur humor ſeroſus & lacteus, ſi revera eſſet ulla hujuſmodi preſſura elaſtica qualis fingitur.* The Reply is eaſie, that the preſſure of the ambient Air, which is a fluid more ſubtil than Butter-milk, will as well hinder the ſtarting out of that liquor as of the parts of the Butter it ſelf: As he will eaſily grant, that attentively conſiders the nature of the thing, and remembers how Air keeps Water from running out at the little holes of a Gardeners Watering-pot clos'd at the top. What the Objector adds about the extruſion of what he calls a ſubtiler Element (ſuppoſed to be harbour'd in the Butter) by the preſſure of the Atmoſphere, in caſe it had any

* p. 142.

any, I think it would not be difficult to anſwer, if we conſider'd, that a great and undeniable preſſure applied to water does not ſenſibly condenſe it, or deprive it of its fluidity, becauſe of the groſsneſs and ſtrength of its parts. But the Argument being but tranſiently mention'd by the Author, and grounded upon a Carteſian ſuppoſition that I never employed, I leave it to thoſe that may think themſelves concern'd (which I am not) to make a ſolemn anſwer to.

And whereas our Learned Examiner ſuperadds, * *Quod tametſi butyri maſſa in diſci lignei ſpeciem reducta, cujus margo centum vicibus areâ ſit minor, interque duas laminas ligneas ejuſdem formæ ac latitudinis poſita, filis ſuſpenderetur in aere tanquam in lance, ita ut preſſura aeris elaſtica quà ab infra, quà de-* * p. 141.

ſuper ducentis fere vicibus exceſſura ſit preſſionem in marginem butyri, butyrum tamen nihilo arctiùs comprimetur per vim aeris elaſticam, nec aliter his afficietur quàm antea: He ſeems not to have ſufficiently conſider'd the Laws of the Hydroſtaticks, according to which, ſuppoſing the preſſure of the Atmoſphere that he rejects, the Butter ought not to be deprived of its ſhape. For the preſſure of the ambient Air, being equal on all ſides, if we ſuppoſe the ſuperficies of the Butter to be diſtinguiſh'd into a multitude of little equal portions, each of theſe, whether they be ſcituated Horizontally, or on the edges, can be preſs'd againſt but by an Atmoſpherical Pillar equal to its Baſis; and the Horizontal portions, if I may ſo call them, cannot be thruſt out of place, without there be at the ſame time ſqueez'd out ſome of the Lateral portions,

tions, which yet cannot be ſo diſplaced, becauſe they alſo are with equal force preſs'd (inwards) by little aerial Pillars, whoſe Baſes are contiguous to them, and bear againſt them. Which Anſwer, though of it ſelf ſufficient, may be much confirm'd by the Inſtance, you will hereafter meet with, of a lump of Butter that kept its irregular ſhape, in ſpight of a great and manifeſt preſſure of the water that ſurrounded it.

And this Anſwer may ſuffice to diſprove, what the Doctor annexes in the beginning of the 13*th* Paragraph, about the vaſt exceſs of Preſſure, which the Air exerciſes upon the flat and Horizontal ſurfaces of the above-mention'd lump of Butter, in compariſon of the preſſure the Marginal parts of its ſurface can be expoſed to. What he adds, and illuſtrates with a Scheme, about the hands being

aſſiſted with the preſſure of the Air, it concerns not me to anſwer. But whereas among the places where the Elaſtical power of the Air is underſtood not to reach, he reckons a Pail full of water, with a lump of butter put in it; he ſuppoſes that, which our Hydroſtaticks will by no means allow, and which is diſproved by ſeveral both of our former Experiments, and by thoſe you will meet with in the following Papers. By which it appears, that the preſſure of the Atmoſphere is exerciſed, as indeed I do not ſee what ſhould hinder it from being, even upon Bodies that are quite immers'd under water; and by which, added to what has been hitherto diſcours'd in anſwer to the Learned Doctors Objections, you will eaſily judge, how deſervedly he ſhuts up the Arguments, we have been examining, with this Conclu-

ſion.

ſion. * *Adeo ut extra omnem controverſiam poſitum videatur, quòd nulla eſt ejuſmodi vis elaſtica in aere, qualem è doctis nonnulli ſupponunt, multoque minus tam fortis ut centum librarum pondus ſuperet. Quod erat Demonſtrandum.*

* p. 143.

CHAP. V.

BUt this is not all the Doctor urges againſt me in this Chapter; for in the 14th Paragraph he ſeconds his former argument by another, drawn from this Experiment of mine, That having taken two round Marbles, whoſe ſurfaces, that were to be contiguous, were as well ground very flat as carefully poliſh'd; and having placed them one directly upon the other, they did in a horizontal

poſture ſo firmly cohere, without the help of any Glue, or viſcous Body, that the upper Marble being pull'd up, would take up the lower, though clogg'd with a weight of fourſcore and odd pounds.

See the Hiſt. of Fluidity and Firmneſs, p. 222. of the ſecond Edition.

This Experiment, when I many years ago firſt publiſh'd it, I referr'd to the action of the Atmoſphere, which preſſing equally and ſtrongly againſt the ſurfaces of both the Marbles, except where they were contiguous, the higher could not be drawn directly upwards from the lower (and conſequently muſt be follow'd by it) by a leſs force than that which was equivalent to the weight of as great a Cylinder of the Atmoſphere as lean'd upon the upper Marble.

This Experiment thus explain'd, though it hath been judged a very favourable

vourable one to the Hypotheſis on whoſe behalf I alledged it, does yet to the juſtly famous Doctor ſeem a very conſiderable Argument againſt it, though for this judgement of his he urges only this reaſon, That if the force, with which the Air preſſes the lower Marble againſt the upper, be able to ſuſtain that Marble, though clogg'd with the great weight above mention'd, the ſame preſſure of Air would much more eaſily ſupport a Plate of wood brought to a true plain, and not loaded with any weight, if the wooden Plate were ſubſtituted to the lower Marble, and inſtead of it applied to the upper.

But ſince the Experiment, as I propoſed it, did upon tryal ſucceed very well, it had not been amiſs if the Learned Examiner had conſider'd it as it was really and ſucceſsfully made, and ſhewed why the preſſure of the ambient

ambient Air was not able to hinder the ſeparation of the Marbles: And his needleſs ſubſtitution of a Wooden plate inſtead of the lower Marble eaſily ſuggeſts a ſuſpition, that there may lie ſome fallacy, though not intended by him, in the variation he propoſes of the Experiment. And he ſeems to have himſelf had thoughts of this kind, by taking notice, that it may be anſwer'd on our behalf, that a Wooden Plate cannot be ſo exactly applied to the upper Marble, but that there will be a little Air intercepted between it and the bottom of that ſtone. And though having granted that it may be ſo, he employes two pages to ſhew, that this intermediate Air could not keep the preſſure of the Atmoſphere from ſupporting the unclogg'd plate of wood, if it had been That preſſure, which, when there was no ſuch intermediate Air, had ſuſtain'd the

the lower Marble with all the appendant weight; yet I confeſs his Proofs ſeem not to me to be anſwerable to the Aſſurance he uſes in ſpeaking of them. His Examples taken from Gunpowder and Wind, you will eaſily judge not to be very proper, where we are not conſidering a force that acts by a ſudden and vaniſhing *Impetus*, but a conſtant and equal preſſure. And as to his other Inſtance, which is taken from five men that thruſt againſt the ſixth (ſtanding with his back to a Wall) who is but as ſtrong as any one of them: I anſwer, that neither is this example near enough of kin to our caſe. For each of theſe five men is ſuppoſed to have an equal power of thruſting, proper to himſelf, and independent from all or any of the other four. And the ſixth man is likewiſe ſuppoſed to reſiſt but by his own ſingle force, without

out having his power of reacting increas'd by the force wherewith the others thruſt againſt him. But in our caſe the thing is quite otherwiſe; for ſuppoſing that ſome aerial particles be ſo placed that a ſolid Body hinders them to recoil or expand themſelves, we are to conſider, that, as the contiguous corpuſcles of air preſs againſt them not by their own ſingle weight or preſſure, but as they tranſmit the action of all the other particles of the air which by their weight or preſſure thruſt them on; ſo the aerial particles, contiguous to the ſolid Body, reſiſt not barely by that force which they would have if they were not compreſs'd, but by vertue of the Springineſs they acquire upon the ſcore of the forcible inflection they ſuſtain from the action of the corpuſcles, that either mediately or immediately thruſt againſt them; and conſequently, in pro-

proportion to that external force, the Elaſticity of theſe compreſs'd Particles will be increaſed, as we ſee that a Bow or other Springy body, the more it is bent by an external force, the greater power it has to reſiſt further compreſſion. Upon which grounds it need to be no wonder, that a ſmall portion of Air, being almoſt included in a ſolid Body, and having for ſome (though but very little) time been expoſed to the outward air, ſhould be capable of reſiſting the preſſure of as much of the whole Atmoſphere, as can come to preſs againſt it. For, this preſſure of the Atmoſphere being continual, if the Springineſs of the aerial particles were not now great enough to reſiſt that preſſure, they muſt neceſſarily have been beforehand inflected or compreſs'd by it, till the endeavours of the one and the other were reduced to an Æquipollency.

lency. Of which I ſhall give you an inſtance in ſo obvious a body as a Bubble at the top of water: For though there be but a little Air included in a very thin and tranſparent film of water; yet this little air is ſo well able to reſiſt the weight of all the Atmoſphere that can come to bear againſt it, that all the preſſure of it is not able to make the film ſhrink, or become wrinkled; which it would do, if the corpuſcles of the Internal air were not reduced to a Springineſs, which makes its power of reſiſting equal to the endeavour of the External Atmoſphere to compreſs it. And to let you ſee, that we may well conceive ſuch a Springineſs of the air included in the Bubbles, I have elſewhere related, how by barely withdrawing the preſſure of the ambient air from Glaſs-Bubbles hermetically ſealed with air in them not compreſs'd beyond

beyond its uſual ſtate, the Spring of the Internal air would make the Bubbles fly in pieces: And this will happen to ſtronger Glaſſes than Bubbles, as you will find in one of the former Experiments *.

And if we would illuſtrate what we are debating of by an Example; it ſhould not be by conſidering, as the Doctor does, the endeavour of five men againſt the ſixth that hath his back to the Wall; but that of five Bladders full of air, pil'd up, and reſting upon a ſixth. For in this caſe, whatever force or power of preſſing we ſuppoſe in the incumbent Bladders, they all bear jointly upon the lower, which *continuing* at a ſtand, muſt thereby be ſo compreſs'd as to be able to reſiſt their joint endeavours, as 'tis manifeſt; becauſe otherwiſe it would not continue in that ſtate, but be

* See the Tract about the Preſſure of the Airs Spring on bodies under water.

be farther compreſs'd; which is againſt the ſuppoſition.

This Notion about Preſſure and Reſiſtance I have the more particularly deduced, becauſe I found many modern Naturaliſts, and even Hydroſtaticians themſelves to be great ſtrangers to it. For which reaſon I ſhall add, that I have evinc'd it by purpoſely deviſed Experiments in the Continuation of the Phyſico-Mechanical Experiments * about the Air. Were it not for this, I ſhould perhaps have ſpared my ſelf the labour of ſetting down theſe thoughts as not neceſſary to the ſolution of the Doctors Objections. For he admits a Layer, or (as he aptly ſpeaks) an *Area* of Aerial Particles to be interpoſed between the upper Marble and the Wooden Plate; and therefore the flatneſs and ſtiffneſs of thoſe two Bo-

* Exper. 25. and elſewhere.

dies

dies muſt keep them from an immediate contact as well at the edges as by the help of the ſame *Area* they do elſewhere; and conſequently that interpoſed Air may communicate with the ambient Air. From whence the Laws of the Hydroſtaticks (which I have elſewhere ſhewn) will allow me to conclude, that the weight of the Atmoſphere endeavours to depreſs the upper ſurface of the wooden Plate; and ſo what the Examiner urges of the inconſiderable reſiſtance, that the few Aerial Particles interpoſed between the flat Bodies can make to the great preſſure of the Column of Air that thruſts the Woodden againſt the Marble Plate, would not conclude, though our former anſwer could not have been made; ſince the reſiſtance, made by the interpoſed Aerial Particles to the preſſure upwards of the Atmoſphere, is not in our preſent ſup-

poſition made by thoſe Particles alone, but by the weight of the lateral and ſuperior part of the Atmoſphere exerciſed by the intervention of theſe Particles. Which being ſo, what the Learned Doctor adds, that the weight of the wooden Plate it ſelf is here of no conſideration, muſt needs be a miſtake. For the two equal Atmoſpherical Preſſures, the one againſt the upper ſurface of the woodden Plate, and the other againſt the lower, countervailing and conſequently fruſtrating the endeavour of each other, the gravity of the wood it ſelf will ſuffice to make it fall, as well as if it were preſs'd againſt by neither of them. And from this Diſcourſe you will eaſily judge, whether the Doctor had reaſon to ſay as he does,* *Quam ab omni ratione (igitur) abſonum eſt, ut ſuperficies illa ſive area aerearum particularum, quæ inſinuant ſe*

* p. 146.

ſe laminam ligneam inter & marmor, ſolidam columnam hujuſmodi particularum, vi elaſtica ſurſum enitentium, contra laminam ligneam obnitendo vincat, ipſamque laminam in terram deturbet.

CHAP. VI.

WHat he adds in the *ſixteenth* number againſt thoſe that fancy the Aerial Particles to be endow'd with Perception, and to act with Deſign *pro re nata*, does not at all concern me; and what he adds in the next Paragraph, wherewith he concludes his twelfth Chapter, I ſhall altogether paſs by, as far as it concerns the extravagant conceit he oppoſes. But becauſe at the cloſe of the Paragraph, he makes an Inference,

which compriſes our Opinion alſo; ſince he concludes, that the Experiment by him alledged, * *Certiſſimum eſt indicium, particulas Aerias nec cum conſilio nec ſine conſilio inferius marmor ſuſtinere nec ſuffulcire:* It will not be amiſs to ſhew, that our Opinion is undeſervedly included in the Inference; which I ſhall do by briefly ſolving the Phænomenon the Doctor layes ſo much weight on. For if we conceive with him, that the two flat Marbles formerly mention'd be ſuſpended, and that to the lower of them a flat woodden plate of the ſame ſhape and extent be applied; I ſee no cauſe to wonder, why the two Marbles ſhould ſtick together, and not the lower of them to the woodden plate. For, as I lately noted, there being an Area or Bed of Aerial Particles interpos'd betwixt the Marble and the Wood, the weight

* p. 150.

weight of the Atmoſphere, exerciſed by the intervention of thoſe Aerial Corpuſcles, ought to be æquipollent to the preſſure of the Atmoſpherical Cylinder, that bears againſt the lower ſurface of the Plate; which conſequently by its own weight muſt drop down: whereas there being no ſuch Layer of Aerial Particles interpoſed betwixt the two Marbles, the preſſure of the ambient Atmoſphere, which touches them every where, ſave where their poliſh'd ſurfaces are contiguous, muſt keep them ſtrongly coherent.

I preſume I need not mind you, that hitherto I have diſcours'd upon ſuppoſition, that the Doctor *experimentally* knows, what he delivers concerning the Non-adheſion of an exactly ſmooth woodden Plate to a Marble one: *And* upon his conceſſion, that, becauſe of the want of

ſufficient congruity between the ſurfaces of two Bodies, there is a bed of Aerial Corpuſcles interpoſed between them. But now I think, it will not be unfit to take notice to you, that though, to illuſtrate on this occaſion a ſubject that is generally ſo little underſtood, as the exerciſe of Preſſion among fluid Bodies, I have anſwer'd my Learned Adverſaries Objections, as if I had nothing more to ſay for my Explication of the Suſpenſion of coherent Marbles, than what I many years ſince deliver'd in the little Tract by him cited; yet I have ſince abundantly confirmed that Explication by the 50*th* of the Experiments publiſh'd in my *Continuation*; which if the Doctor had been pleaſed to read, perhaps he would have received the ſame ſatisfaction that other Learned men have done: ſince there I experimentally ſhew, that

that the undermoſt Marble without the accuſtomed Clog, would, upon the bare withdrawing of the ſuſtaining air, drop off from the upper. And whereas the two Marbles in our Vacuum would not cohere; as ſoon as the formerly excluded Air was let in upon them, it did by its ſupervening preſſure make them ſtick together very ſtrongly.

The Second Section.

CHAP. I.

I Proceed now to the second of those two Chapters, that I am interess'd to consider, in which the Learned Examiner is pleased to attaque three or four of my Hydrostatical Opinions and Explications; in the defence whereof, I hope, I shall be the less put to exercise your Patience, because the Learned Doctor himself is pleased to grant me almost as much as I need desire concerning the Truth of the *Hypothesis*, whereon my Paradoxes and Explications are founded. For whereas the main thing I suppos'd in my Hydrostatical Papers, is, that in water, though stagnant, the superior parts do *actually*, though not always

pre-

prevalently, gravitate upon the inferior, or (if you will) *preſs* upon them, even when they do not ſenſibly *depreſs* them; the Doctor in divers places allows this *Hypotheſis* to be conſonant to the Principles of the Mechanical Philoſophy; and accordingly having ſhew'd, that in a ſuſpended tub of water the whole liquor gravitates upon the bottom of the tub; He ſubjoyns, * *Jam verò cum tota hæc aqua conſtet ex particulis aqueis non compactis vel concretis, ſed ſolutis à ſe invicem, impoſſibile eſt ut omnes fundum ſitulæ premant, niſi infima quæque ab omnibus ſuperioribus premantur, quemadmodum clarè demonſtravimus in ſecunda ſectione hujus capitis; nempe, ſi nullæ cauſæ niſi purè Mechanicæ (quales ſunt Motus localis, Magnitudo, Figura, &c.) in edendo hoc Phænomeno ſe intermiſcent.*

* P. 161.

And elſewhere in the ſame Chapter

ter he ſpeaks thus of the gravitation of liquors (towards the cloſe of the ſecond Paragraph.) * *Neceſſe utique eſt, ut partes ſingulæ gravitent, cum totius ſit gravitatio, ſi non ſit aliquid immateriale Principium in rerum natura,* &c. And adds, at the beginning of the next Number; *Atque ſanè huic externi motûs Hypotheſi, & gravitationis Elementorum in propriis locis inde neceſſariò emergentis, apprimè conſonum eſt primum illud Experimentum, quod Scriptor profert in Paradoxis ſuis Hydroſtaticis.*

* p. 152.

And now, Sir, I preſume you do not much wonder, if I think theſe conceſſions reach the main thing I pretend to. For though I do as freely and heartily, as the Doctor himſelf, (who, I dare ſay, does it very ſincerely,) admit or rather aſſert an Incorporeal Being that made and governs the world; yet all that I have en-

endeavour'd to do in the Explication of what happens among Inanimate Bodies, is to ſhew, that, ſuppoſing the World to have been at firſt made and to be continually preſerv'd by Gods divine Power and Wiſdome; and ſuppoſing his General concourſe to the maintenance of the Laws he has eſtabliſhed in it, the Phænomena, I ſtrive to explicate, may be ſolv'd Mechanically, that is, by the Mechanical affections of Matter, without recourſe to Natures abhorrence of a *Vacuum*, to Subſtantial Forms, or to other Incorporeal Creatures. And therefore, if I have ſhewn, that the *Phænomena*, I have endeavour'd to account for, are explicable by the motion, bigneſs, gravity, ſhape, and other Mechanical affections of the ſmall parts of liquors, I have done what I pretended; which was not to prove, that no Angel, or other immaterial

terial *Creature* could interpose in these cases; For concerning such Agents, all that I need say, is, that in the cases propos'd we have no need to recurr to them. And this, being agreeable to the generally own'd rule about *Hypotheses*, that *Entia non sunt multiplicanda absque Necessitate*, has been by almost all the modern Philosophers of different Sects thought a sufficient reason to reject the agency of Intelligences, after *Aristotle* and so many Learned men, both Mathematicians and others, had for many ages believ'd them the Movers of the Celestial Orbs.

CHAP.

CHAP. II.

But you will tell me, that the Doctors Conceſſions will not avail me, ſince he urges againſt the Gravitation of the Elements in their proper places, which (gravitation) he would have to be ſuſpended by his Incorporeal Principle, an Experiment, which he ſays is moſt manifeſtly repugnant to our *Hypotheſis*. He conceives then, that in a tub or pail full of water with a perfectly Cylindrical cavity, whoſe *Diameter* is of ſixty two parts, there is violently kept at the bottom, by the help of a ſtick, a round Plate of wood, whoſe *Diameter* amounts but to ſixty one of thoſe parts; and that, as ſoon as ever the ſtick is removed, the woodden plate will emerge to the top and float.

float. *Quod* (ſays he) *prorſus impoſſibile eſſet, ſi omnes partes aquæ ab* (F G) *ad* (H J) *non ſolùm junctim fundum vaſis, ſed ſingulæ ſingulas in eadem ſerie ſubjectas actu premerent.* To which aſſertion he immediately ſubjoyns this Argument to prove it by; * *Cum Diameter laminæ ligneæ* (H M) *partes 61. habeat æquales, Diameter vaſis* (H I) *habeat 62, manifeſtum eſt, quod ſuperficies fundi vaſis ad ſuperficiem laminæ ſe habet ut 3844. ad 3721; quorum differentia eſt 123. Itaque rotundum intervallum inter latera vaſis & marginem laminæ ligneæ habet ſe ad aream laminæ ut 123. ad 3721, hoc eſt, area laminæ ligneæ excedit aream dicti intervalli pluſquam triginta vicibus. Ac proinde aqua incumbens ligneæ laminæ excedit magnitudine aquam incumbentem dicta intervallo inter marginem laminæ & latera vaſis*

* p. 155.

plus

plus quàm triginta vicibus, pondúſque ſive preſſio hujus, alterius pondus preſſionémque vincit pluſquam triginta vicibus. Adeò ut impoſſibile ſit, ut aqua incumbens prædicto intervallo ita premat aquam ipſi ſubjectam, ut hujus vi ſublevetur lamina, quam vis tricies major deprimit. Quod (ſays he, by way of inference) *æque abſonum atque abſurdum Phænomenon eſſet*, &c.

How little this Ratiocination agrees with the Experiments I have formerly told you of, about the caſes wherein Light bodies will be detain'd under water, or emerge to the top of it, you will eaſily perceive, if you compare the one with the other, which you may quickly do, if you pleaſe to compare the Doctors diſcourſe with the following Narratives of thoſe Tryals*, to which alone I might therefore refer you.

* See the Tract of the Poſitive or Relative Levity of Bodies under water. *Exp.* 1. *&c.*

But

But yet in the mean time, you may, if you think fit, conſider a little, whether the Argument, whereon the Doctor lays ſo much ſtreſs, be any more than a *Paralogiſm.*

Firſt then, ſince according to his computation the *Area* of the interval between the ſides of the Veſſel and the edges of the round boards, is 123 of ſuch parts, whereof the *Area* of the board amounts to 3721; 'tis evident, that there muſt be room enough for the water to paſs between the ſides of the veſſel and the edges of the board, which is ſuppos'd on all hands to be of ſome wood lighter *in ſpecie* than water, ſince elſe it would not emerge upon the withdrawing of the ſtick.

Next, this Board or woodden Plate is not here intimated or ſuppoſed to be (and indeed in practice can ſcarce be) made exactly congruous to the bottom of the Veſſel, and conſequently the

the water may get in between them; for which cauſe 'tis neceſſary to keep the woodden Plate forcibly down with a ſtick, which elſe were needleſs. And conſequently this interpoſed water will communicate with the laterally ſuperior water in the Veſſel, which ſuperior water may, according to the Laws Hydroſtatical, by the intervention of the interpoſed, exercise its preſſure upwards againſt the lower ſurface of the woodden Plate.

Thirdly, the Doctors Scheme allows and aſſiſts us to conceive, (which we may do however,) an imaginary Plane of water to be parallel to the bottom of the Veſſel, and to paſs along the bottom of the Board; ſo that, of the water that lies between this Plane and the bottom of the Veſſel, one part is cover'd by the woodden Plate; and the other, between the edges of that and the ſides of the

tub, is cover'd with the incumbent water only.

CHAP. III.

THeſe things being premis'd, I thus argue: 'Tis manifeſted by Hydroſtaticians after *Archimedes*, that in water, thoſe parts that are moſt preſs'd, will thruſt out of place thoſe that are leſs preſs'd: which both agrees with the common apprehenſions of men, and might, if it were needful, be confirm'd by Experiments. 'Tis alſo evident, that that part of the above-mention'd imaginary Plane, that is cover'd by the woodden Plate, muſt be preſſed by a leſs weight than the other part of the ſame Plane; becauſe the wood being bulk for bulk lighter than water, the aggre-

aggregate of the wood and water incumbent on the cover'd part of the ſame Plane muſt be lighter *in ſpecie*, than the water alone that is incumbent on the uncover'd part of the ſame Plane; and conſequently this uncover'd part being more preſs'd than the other part of the Plane, the heavier muſt diſplace the lighter, which it cannot do but by thruſting up the board, as it does, when the external force that kept it down is removed. And, to add this upon the by, this greater preſſure againſt the bottom than againſt the top of bodies immers'd in water ſpecifically heavier than they, is a true reaſon of their emerſion, as I have elſewhere ſhewn. So that there happens no more in this caſe than what uſually happens in the aſcenſion of bodies in liquors ſpecifically heavier than themſelves, on the account of the

newly mention'd difference of Preſſure. And 'tis with an (expreſs or ſuppos'd) exception of ſuch a difference, which in many other caſes may be ſafely neglected, that (which I deſire you to take notice of,) in moſt places of this diſcourſe I ſpeak of the Preſſure of ambient Fluids on immerſed Solids as uniform or every way equal.

'Tis true, that according to the Doctors ſupputation, if the ſolid Cylinder, conſiſting of the woodden Plate, and all the water directly incumbent on it, were put into an ordinary ballance, it would there many times out-weigh the hollow Cylinder of water alone that leans upon the uncover'd part of the imaginary Plane. And that is it that ſeems to have deceiv'd the Learned Doctor. But there are divers Hydroſtatical Caſes, wherein the *Phænomenon* depends not ſo much

much upon the abſolute weight of the compared Bodies, as upon their reſpective and their ſpecifick Gravity; on whoſe account it is, that a ſmall Pible, for inſtance, that weighs not a quarter of an Ounce, will readily ſink to the bottom of the river, on whoſe ſurface a log of wood of a hundred pound in weight will float. 'Tis a Rule in Hydroſtaticks, that when two portions of water or any other Homogeneous liquor preſs againſt each other, the prevalency will goe, not according to the abſolute weight, but the perpendicular height of thoſe Portions. And accordingly we find, that if a ſlender pipe of glaſs, being fill'd with water, have its lower orifice unſtop'd at the bottom of a veſſel of water, which contains much more of that liquor than the pipe; yet if this laſt named water were, for inſtance, two foot high, and that in

the Veſſel but one, the water in the pipe will readily ſubſide, till it come almoſt to a level with the external water, though it cannot do ſo without raiſing the whole maſs of water that ſtagnated in the veſſel.

And now I ſhall ſubjoin an Experiment, which, though at firſt it may ſeem ſlight, and was made in leſſer glaſſes & quantitys than I would have imploy'd if I could have procur'd better Accommodations, has the advantage of requiring no curious inſtruments, and yet I hope will ſerve for an ocular proof of the fallaciouſneſs of that reaſoning the Doctor is ſo ſtrangely confident of.

We took an open mouth'd glaſs, ſuch as ſome call Jarrs, and Ladys often uſe to keep ſweet meats in, which was three inches and a half or better in *Diameter*, and ſomewhat leſs in depth; and had the figure of its cavity Cylin-

Cylindrical enough. Into this having put ſome water to cover the protuberance, wont to be at the bottom of ſuch glaſſes, we took a convenient quantity of Bees-wax, and having juſt melted it, we poured it cautiouſly into the glaſs, warm'd before-hand to prevent its cracking, till it reach'd to a convenient height. This veſſel and the contained liquors we ſet aſide to cool, in expectation, that when the heat, that had dilated the wax, was gone, it would ſhrink from the glaſs, and conſequently leave a little interval every where between the concave ſuperficies of the veſſel, and convex of the harden'd wax; which accordingly came to paſs, and ſav'd me the labour of getting the wax ſhap'd for my purpoſe with tooles; which might have been done but not without trouble and leſs exactneſs. And now 'twas eaſie for me to try the ex-

periment I deſign'd; for, pouring in warily ſome water between the glaſs and the wax, ſo that it fill'd all the interval, left between thoſe two bodys both at the bottom and the ſides, the wax was made preſently to float, being viſibly lifted up from the bottom, and its upper part appearing a little above the level of the water, which was no more than I did, and had reaſon to expect, according to the true Principles of Hydroſtaticks. For water being ſomewhat, though but little, heavier, *in ſpecie*, than wax, and that which was poured into the bottom and ſtagnated there, being preſs'd by the collateral water, every way interpos'd between the concave part of the Glaſs and the convex of the Wax (ſo that this collateral liquor anſwer'd what I lately called a hollow Cylinder of water in the Doctors Experiment) that part

of

of the ſtagnant water, that was lean'd upon by the wax, being leſs preſs'd than the other part of the ſame ſtagnant water was by the water incumbent on it; this latter muſt diſplace the former, which it could not doe but by raiſing up the wax that lean'd upon it. And yet this collateral water was ſo far from being heavier than the wax its preſſure impell'd up, that both the collateral, and the ſtagnant water all together, being weigh'd in good ſcales, amounted to little above a quarter of the weight of the wax, which happen'd by reaſon of the narrowneſs of the Veſſel, which, if it had been wide enough, I doubt not but the experiment would have ſucceeded, though the wax had outweigh'd the collateral water ten times more than in our experiment it did. But that the ſolid body exceeded almoſt four times the weight, not

not onely of the collateral but the ſtagnant liquor too, does ſufficiently overthrow the Doctors ratiocination. Whoſe fallaciouſneſs will yet further appear by two other improvements, among others, which I made of one Experiment.

For I. though we pour'd in more and more water, as long as the Veſſel would contain any, the Cylinder of wax was but lifted higher and higher from the bottom of the glaſs, but did not appeare rais'd more than at the firſt, above the upper ſurface of the water; which argues, that 'twas not at all the Quantity of the inferior water, which was continually increas'd, but the preſſure of the collateral water, which continued ſtill at the ſame height in reference to that wax, that caus'd the elevation of the body.

And II. to manifeſt yet more clearly

clearly the Doctors miſtake, I deviſed the following tryal. We took a round plate of Lead about the thickneſs of a ſhilling, and having made it ſtick faſt to the bottom of the Cylinder of wax, to make this body ſink the more directly, we placed one after another upon the upper part of the wax divers grain weights (firſt wetted to keep them from floating) till we had put on enough to make the wax ſubſide to the bottom: For the facilitating whereof we had par'd off its edges; by this means, the glaſs having been at firſt almoſt fill'd with water, there ſwam about an inch or better of that liquor above the upper ſurface of the wax. And laſtly, we took off by degrees the grain weights that we had put on, till we ſaw the wax, notwithſtanding the adhering Lead, riſe, by degrees, to the top of the water, above

above which ſome part of it was viſibly extant.

From this experiment I thus argue: 'Tis manifeſt, that, according to the Doctors ſuppoſition, here was incumbent upon the wax a *Cylinder* of an inch in height and of the ſame *Diameter* or breadth with the round ſurface of the wax, whereas upon the removing part of the water, that lay at the bottom when the wax began to riſe, there was incumbent no greater weight than that of the collateral water, and as much of the ſuperior and ſtagnant, as was directly imcumbent upon that collateral water (and would have deſerv'd the ſame name, if we had ſuppos'd the convex ſurface of the wax to have been continued upwards as high as the glaſs reach'd.) But now, whereas, according to the Doctors ratiocination, this Cylinder of water incumbent on the wax,

wax, being an inch deep, and a good deal above three inches broad, muſt preſs the wax with a greater weight by ſeveral times, than that which the lateral and hollow *Cylinder* of this ſtagnant water could have upon the reſt of the collateral water; yet the height of this aggregate of collateral waters being the ſame with that of the wax and the water ſwimming upon it, the difference of the preſſure was ſo ſmall, that barely taking off a weight of four or five grains, the wax would, notwithſtanding the preſſure of the water incumbent on it, be impell'd up and made to float: And by the like weight, put again upon it, it would be made to ſink, and by another removal of ſuch a weight, (for I purpoſely reiterated the tryal more than once,) it would, though ſlowly, reaſcend. And theſe *Phænomena* do ſo much depend upon a Mechanical

æqui-

æquipollence of preſſure, that even four grains would not have been neceſſary to make the wax riſe or ſink, if it had not been for ſome little accidental impediments, that are eaſily met with in ſuch narrow glaſſes; for otherwiſe in a larger Veſſel we have made the ſame Lump of Wax readily enough ſink or float, by the putting in or taking off a ſingle grain or perhaps leſs.

By this you may ſee, that for the Regulation of Hydroſtatical things, Nature has her ballance too as well as Art, and that in the ballance of Nature the Statical Laws are nicely enough obſerv'd.

You may alſo take notice, upon the by, how little the weight of the Cylinder of water upon a body immers'd in ſtagnant water is conſiderable, whilſt there is a preſſure of collateral water to counterballance it:

it; ſince in this laſt tryal, though the *Cylinder* of incumbent water did continually increaſe or decreaſe in length, whilſt the lump of Wax was ſinking or emerging; yet the ſame deſpicable weight of a grain or leſs, that was juſt able to depreſs it beneath the upper ſurface of the water, did by its preſſure or removal procure its ſinking to the very bottom, or riſing again to the top, and on both occaſions with an equal ſlowneſs, bating that little acceleration of motion, that ought to happen upon another account, and which therefore is to be obſerv'd in the wax, during its riſing as well as during its ſinking.

CHAP.

CHAP. IV.

SOme other *Phænomena* I produc'd, by varying the hitherto menti-on'd experiment, which are very favorable to our Notions about Hydro-ſtaticks. But, ſince they do not directly concern the preſent Controver-ſie, I ſhall in this place only annex a couple, the former whereof affords an eaſie confirmation of that Paradox, which we lay as the ground of divers others, and the contrary where-of is maintan'd not only by Doctor *More*, but by many other famous and Learned men, namely, that in ſtagnant water the upper parts do actually preſs the lower.

Wee took then a very ſlender pipe of glaſs, whoſe Cavity was nar-rower than that of an ordinary Goos-quill, that heterogeneous Liquors may

may not be able to get by one another in it. This Pipe near one end was bent upwards like a Syphon, that it might have a ſhort leg as Parallel as the Artificer could make it to the longer. Into this crooked Pipe we put a little oyl, and then held it perpendicularly in a ſomewhat deep and wide-mouth'd Glaſs fill'd partly with Water and partly with a Lump of Wax, of the bigneſs and ſhape of that already mention'd; that ſo the preſſure of the incumbent Water upon the open orifice of the ſhorter Leg, might impel the oyl into the longer Leg, ſomewhat above the ſurface of the water in the Veſſel; which 'twas convenient ſhould be done, that we might the better ſee the motions of the Oyl, and which we knew *muſt* be done by the courſe we took; both becauſe Oyl is lighter *in ſpecie* than Water, and conſequent-

ly required not an equal height of Water to counterballance it; and becauſe in very ſlender Pipes, Water is wont to aſcend a little above the Level of the External Water, whereinto they are immers'd. The Pipe being, as was ſaid, held upright, 'twas eaſie to take notice by a mark, fix'd on the outſide, to what height the Oyl reach'd in it.

Now if we conceive a Horizontal Plane, Parallel to the bottom of the Veſſel, to paſs by the Baſis of the floating Wax, 'tis evident by what has been formerly ſhewn, that, of this Imaginary Plane, that part on which the Wax is incumbent is as ſtrongly preſs'd by the weight of the Wax, as the Lateral part of the ſame Plane is by the weight of the Water incumbent on it; (otherwiſe theſe Preſſures would not be æquipollent, but the Wax would be raiſed:) And conſequently

ly that part of this Plane, that is placed directly over the Orihce of the ſhorter Leg of the Pipe, is no more preſſed, than any equal portion of that part of the ſame Plane that is cover'd by the Wax. This Body being taken out of the Water, the Liquor ſubſided a great way in the Veſſel, and ſo did proportionably the Oyl in the longer Leg of the Pipe. And laſtly, having weigh'd out in a good pair of Scales as much Water, as we found the Wax to amount to, this Liquor was, inſtead of the Wax, poured into that which remained in the Glaſs; whereupon the Oyl, in the longer leg of the Pipe, was again impell'd up (very near) to the former Marke to which it had been raiſed by the Wax. Whence we may gather, that the Water newly put in, though in the Air it weigh'd no more than the Wax; yet it did as much

preſs the Water, that lay beneath the foremention'd Imaginary Plane, and conſequently that, which was directly over the ſhorter Leg of the Pipe, as the Wax, that had been taken out, had done. And ſince we have already proved, that the Wax did conſiderably preſs that Plane, it ought not to be denyed, that the Water alſo (which inſtead of it was able to impell up the Oyl in the Pipe) did in like manner preſs that Plane; and conſequently that Water may gravitate in Water, as well as a ſolid Body, ſuch as Wax is, can. And this is the *firſt* additional uſe I told you I would make of our Experiment.

But, (to come now to the *ſecond*) there is another *Phænomenon* of it, viz. the abovemention'd tenderneſs of Nature's Ballance, whoſe uſe ſeems to be of no leſs general concernment to the true Doctrine of the Hydroſta-

ticks.

ticks. For, by duely conſidering that *Phænomenon*, and reaſoning a while upon it, we may be help'd to rectifie that plauſible Miſtake, which has long deluded both Philoſophers and Mathematicians, and does yet impoſe on moſt of them; namely, that a Body does not actually gravitate when it does not deſcend. For we have ſeen already, and ſhall further ſhew by and by, that the ſunken Wax and the Braſs grains that lie on it, do actually preſs or gravitate upon the ſubjacent Water and Bottom of the Veſſel on which 'tis incumbent; and conſequently its preſſure being not ſurmounted by that of the Collateral Water, which is unable to raiſe it, muſt be as great, as that of this collateral Water. Therefore, when upon the removal of a ſingle Grain, the Wax with its incumbent weight is made to aſcend, and that but very

ſlowly, 'tis evident, that 'twas ſo far from not gravitating before, becauſe it did not actually deſcend, that it retain'd its Gravity even whilſt it *aſcends*: As may appear *not only* by the ſlowneſs of its motion upwards, proceeding from its being in Nature's Ballance very little leſs heavy than it need be to countervail the preſſure of the Collateral Water; *but* by this alſo, that if but a ſingle grain be laid on it when it begins to riſe, its aſcenſion will be check'd and hindred, which could not be done by the addition of ſo inconſiderable a weight, if the Wax and the adhering Metall did not, even during their aſcent, retain their former gravity, though that were fruſtrated as to the act of deſcending, or ſo much as keeping their ſtation by the prevailing preſſure of the collateral Water: So that, ſince, (as we found) the Wax and adhering Metall

tall amounted to a good deal above 4000 Grains, it did in the Ballance of Nature weigh, whilſt it was aſcending, not ſo much as a 4000*th* part leſs than it did, whilſt it was actually deſcending.

CHAP. V.

I Should beg your pardon, Sir, for having detain'd you ſo long with my Reply to a ſingle Objection of the Doctors, how pompouſly ſoever propos'd; but that I thought it not amiſs to do ſome ſervice to the true Theory of Hydroſtaticks, by taking this occaſion to preſent you ſome things that I thought not unlikely to illuſtrate ſome parts of that Theory; though above what was neceſſary to anſwer the Doctors Argument; to which I confeſs I was troubled to ſee

ſo Learned a man ſubjoin the following concluſion: *Hæc tam luculenta Demonſtratio contra Gravitationem particularum aquæ inter ſe quamvis junctæ ſitulæ fundum urgeant, ſi non ſit vera atque ſolida, equidem nec mei ipſius nec ullius unquam mortalis in poſterum ratiocinii credam.* But I hope he will not be as bad as his word, but will be pleas'd to conſider as well as I do for him, that a man may be very happy in other parts of Learning and of greater moment, that has had the misfortune to miſtake in Hydroſtaticks, a diſcipline which very few Scholars have been at all vers'd in, and about which divers of thoſe few have had the misfortune to err, not only in the concluſions they have drawn, but in the very Principles they have embraced.

To the foregoing Argument the Doctor,

Doctor, though he declares he thinks it needleſs, adds in the 5*th* Paragraph another, taken from the Laſt experiment of my Hydroſtatical Paradoxes, by which he ingenuouſly acknowledges, that I ſeem at firſt ſight to have demonſtrated what I pretend to, about the gravitation of the upper parts of ſtagnant water upon the lower. And I am ſorry that I cannot in return acknowledge, that his objection at firſt ſight ſeem'd to me a cogent one: For, neither at the ſecond nor third peruſal can I clearly diſcern where his Ratiocination lyes, ſuppoſing it to be meant for an anſwer to my experiment. And though I conſulted with ſome Learned Members of the Royal Society, whereof two are Mathematicians, and one his particular friend; yet they all confeſs'd he had not ſufficiently explain'd himſelf on this occaſion, nor could

could they ſhew me to what argumentation I might properly direct my reply. Only one of the Doctors Correſpondents, having ſeriouſly perus'd his diſcourſe and the annex'd ſcheme, told me that what ſeem'd the moſt probable to him, was, that though the Doctor was too Civil to give me, *in terminis*, the Lye; yet he did indeed deny the matter of fact to be true. Which I cannot eaſily think, the Experiment having been tryed both before our whole Society, and very Critically, by its Royal Founder his Majeſty himſelf. But, ſince you have your ſelf ſeen and made it more than once, I need not ſpend words to convince you that the matter of fact is true.

But after I had in vain ſought the Doctors meaning where I expected it, chancing lately to caſt my Eyes on another place, where I ſaw my Scheme repeat-

repeated, I find this paſſage in the Explication he endeavours to give of the *Phænomenon* by his *Hylarchical* Principle; *Cùm verò tam profundè immergitur tubus, ut obturaculum tangat Superficiem V W, vis retractionis Aeris ita augetur ut etiam ponderis appenſi ſuperadditam depreſſionem ſuperet. Videtur igitur quaſi quædam ſurſum-ductio Aeris in tubo contenti, & conformis ac contemporanea aquæ compulſio in obturaculum, quo tam firmiter in os valvulæ comprimitur, ibique cum appenſo pondere ſuſtentatur.* What conſiderable intereſt the ſuppoſed, but unprov'd, Retraction of the Valve or the Air it ſelf can have in this Phænomenon, I confeſs I do not diſcern, not being able to ſee, but that the experiment would ſucceed when try'd *in vacuo*, although all the Atmoſpherical Air were annihilated. But if I miſtake the Doctors

meaning

meaning I am to be excuſed, ſince I do it not willingly, and his own obſcurity has been acceſſary to it. Nor am I very apprehenſive of being unable to defend my account of an experiment, which (as you know) has had the good fortune to recommend the Doctrine, for the Proof whereof I devis'd it, to many Learned and curious Perſons, ſeveral of which were ſufficiently indiſpos'd to admitt it.

And to avoid all miſtakes and diſputes that may ariſe (which I think they muſt do needleſsly) upon the ſcore of the *Valve* imploy'd in our Experiment, I ſhall remind you of another, that I remember I have ſometimes ſhew'n you and divers other *Virtuoſi*, though I remember not whether I have mention'd it in any of my publiſh'd writings. The Summ of this tryal is, that an arbitrary Quantity of Quickſilver, being by Suction rais'd

rais'd into a very ſlender glaſs-pipe, whoſe upper Orifice is ſtop'd with the Experimenters finger, to keep the Mercury from falling before its time, the open end of the pipe with the Mercury in it is thruſt into a competently deep glaſs of water till the little *Cylinder* of Mercury have, beneath the ſurface of the water, attain'd to a depth, that is at leaſt 14 times as great as the Mercurial Cylinder has of *height*. For then, the finger being remov'd from the upper orifice, the glaſs-pipe will be open at both ends, and there will be nothing to hinder the Quickſilver's falling down to the bottom, but the reſiſtance of the *Cylinder* of water, that is under it, which *Cylinder* can reſiſt but by vertue of the weight or preſſure of the ſtagnant water that is ſuperior to it, though but collaterally plac'd above it: And yet this water

water being by the pipe, whoſe upper part is higher than its ſurface, and acceſſible only to the air, kept from preſſing againſt the Mercury any where but at the bottom of the Pipe, and being about a 14*th* part of the weight of an equal bulk of Mercury, it is able at that depth to make the ſubjacent water preſs upward againſt the *Mercury*, which is but a 14*th* part as high as the water is deep, with a force equivalent to that of the gravity wherewith the Mercury tends downwards. And to manifeſt, that this *Phænomenon* depends meerly upon the *Æquilibrium* of the two liquors; if you gently raiſe the lower end of the pipe towards the ſurface of the water, this liquor, being not then able to exerciſe ſuch a preſſure as it could at a further and greater depth, the Mercury preponderating will, in part,

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(more or leſs, as the pipe is more or leſs rais'd) fall out to the bottom of the glaſs. But if, when the Quickſilver is at the firſt depth, inſtead of raiſing the pipe you thruſt it down farther under the water, the preſſure of that liquor againſt the Mercury increaſing with its depth, will not only ſuſtain the Mercury, but impell it up in the pipe to a conſiderable diſtance from the lower orifice of it, and keep it near about the ſame diſtance from the ſurface of the laterally ſuperior water. And this experiment may not only ſerve for the purpoſe, for which I here alledge it; but alſo, if duely conſider'd and applyed, may very much both illuſtrate & confirm the Explication formerly given of the ſeemingly ſpontaneous aſcent of the clogg'd ſucker in our exhauſted Air-pump.

The laſt Argument, the Doctor urges againſt the Gravitation of water

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in what they call its proper place, is deduc'd from what happens to the *Divers*, who in the midst of the Sea, though the salt water of that be much heavier than that of freshwater Rivers, do not find themselves oppress'd, or so much as feel themselves harm'd or compress'd by the vast load of the incumbent water.

But that the Equality of the pressures of an ambient fluid will goe a great way towards the solving of this Difficulty, you will find, by the Experiments and considerations you will meet with in the following * Papers, to which, for that reason, I refer you. And though the Doctor in this same Paragraph objects, *Tametsi hæc pressio æqualis sit, nihil tamen impedit quò minùs subtiliores partes corporis magisque fluidas exprimat & elidat.* I remember I answer'd that

* The Author means the New experiments of the differing pressure of heavy solids, & fluids,

that exception before, by ſaying, that thoſe liquors that he ſuppoſes ſhould be ſqueez'd out, cannot be ſo, becauſe there is as great a preſſure againſt thoſe parts at which they ſhould iſſue, as againſt any of the reſt, if the parts that ſhould be ſqueez'd out be not too ſpirituous and ſubtile, which if they be, I ſhould gladly learn how the Doctor knows that no ſuch minute and ſpirituous particles are really expell'd: eſpecially if that be obſerv'd, which we ſhall ſoon have occaſion to relate, that a ſmall animal, being vehemently compreſs'd in water, ſeem'd a little, though but a little, to ſhrink.

But that we may the more diſtinctly conſider this grand argument, taken from the experience of the *Divers*, that is wont to be employ'd by the Schooles and others for the vulgar Opinion, and is now urg'd by the

Learned Doctor to prove *His*; 'twill be convenient to obſerve, that it does, at once, both propoſe a Queſtion, and contain an Objection, grounded upon the ſurmis'd inſolubleneſs of that Queſtion.

And to begin with the Probleme, *Whence it is, that* Divers *are ſo far from being kill'd or oppreſs'd by the weight of the incumbent water, that they are not ſo much as hurt by it, nay, that they ſcarce feel it at all?* We may take notice, that there is in it ſomewhat ſuppos'd, as well as ſomewhat demanded. For, in the Queſtion 'tis taken for granted, that *Divers*, though at never ſo great a depth, feel no preſſure exerciſed againſt them by the water; which is an affirmation in point of fact, of whoſe truth I make ſome queſtion, for the reaſons I ſhall ere long have occaſion to mention.

But it will clear the way for what

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is to follow; if I here divide the noble and difficult Problem, we are to consider, into two Questions; the *first*, why a *Diver* should not be oppressed and crush'd to death by the pressure of the Incumbent and Ambient water. And the *second*, why at least he should not be made sensibly to feel it by suffering some considerable inconvenience from it.

In answer to the *first* of these Questions, you will easily perceive, that divers things may be pertinently applyed, that you will meet with in the following Paper, to shew the difference betwixt the pressure of *Fluid* and that of *Solid* bodies. And that *de facto* the pressure of water may be exceeding great without destroying an *Animal* quite surrounded with that liquor; I have long since shewn in another * Treatise, by the experiment

* The Author points at the *Appendix* to the *Hydrostatical Paradoxes.*

of a little *Tadpole*, which being, together with the water it swam in, included in a bent Glass seal'd at one end, the animal was not kill'd or sensibly hurt, but only (according to what was lately noted by anticipation) seem'd to shrink into somewhat (and but little) lesser dimensions.

If it be here alledged, that this Experiment makes rather against me than for me, the Learned Doctor having made use of it with a Scheme to explain it in his 16th. *Paragraph*; it will be fit for me to consider his Objection. Having then recited the matter of fact newly deliver'd, he adds, *Quod certè fieri non posset nisi juxta legem quartam contrusio particularum aquæ contra se invicem Principio Hylarchico inhiberetur & eluderetur. Atque hinc fit, ut quamvis Aqua in tubo (A B C) vi trudis (G F) aliquantò facta sit condensatior, partes tamen*

tamen sic compressæ ut propiùs ad se invicem accedant, nihilo inde inter se fiunt comprimentiores. And then subjoining the following passage; *Neque enim sequitur ex earum contactu quod premant se invicem, quandoquidem particulæ, uti fit in duris Corporibus, in unum coalescere possunt & tamen non mutuò se premere*; (Wherein are some things that might be question'd if it were necessary;) He thus pursues his Discourse: *Cùm verò hìc particulæ aquæ sì omninò premerent se invicem, pressura in Gyrinum, columnæ aqueæ, ducentos vel trecentos pedes, æneæ verò, plus viginti vel triginta pedes altæ, pressionem adæquaret, luculentum est indicium quod revera particulæ se invicem non premant. Nam planè est incredibile, columnum æneam pro corpore quidem gyrini latam, sed altam viginti vel triginta pedes & amplius, Gyrinóque*

ad perpendiculum incumbentem omnia viscera tam tenella Gelatinæ non esse elisuram. Notwithstanding which allegation I am apt to think, you will judge the Argument from this experiment to be more probable on my side than on the Doctors. For there being in our case an *animal*, exceedingly much more tender than a man, expos'd to a pressure which he affirms is so great, that if it were exercis'd on the *Tadpole*, it ought to squeeze out all his guts, I think I may pretend to have given a pertinent instance, that a *Diver* may be at a considerable depth under water preserv'd from being crush'd to death by the weight of it. And whereas the *Doctor* tells us, that the cause of the Incolumity of the *Tadpole* is, that the pressure or contrusion of the particles of the water against one another is hinder'd or frustrated by the *Principium Hylar-*

Hylarchicum, I reply; That what I affirm is matter of Fact, and *evident*, (namely, that there was a great external force duly and yet ineffectually applyed to preſs to Death by means of the water the *animal* ſwimming in it;) but that this Mechanical force was ſuſpended or made ineffectual by ſome inviſible and immaterial Agent, is but the Doctors *Hypotheſis*, and a thing, which, whether it be true or no, is at leaſt not manifeſt.

Having ſaid thus much about the *firſt* Queſtion; I now proceed to the ſecond, *Why Divers though at never ſo great a depth complain not of the preſſure of the water, nor ſuffer any harm nor inconvenience by it?*

And here, *Sir*, the Queſtion highly meriting a particular Curioſity, I ſhall not ſcruple in the more full enquiry, I am now entring upon, as well

ſometimes to employ and inlarge particulars already mentioned in the laſt of the following Papers, as oftentimes to ſtrengthen them with new ones. And I ſhall alſo for a while ſuſpend my difference with the *Doctor*, and addreſſing my ſelf to you, who, I am ſure, will allow me that water weighs in water, propoſe, according to my cuſtom, not as a Dogmatiſt, but as an Inquirer, ſome particulars, that may tend to the Solution of a *Problem*, which I take to be as difficult as noble. Not that I doubt but it muſt and will be explicated upon the Mechanical Principles; but *partly*, becauſe the application of them to the Solution will not offer it ſelf to every ſeeker; and *partly*, becauſe we are not yet well furniſhed, either with experiments made on bodies under water, or ſo much as with ſo competent an account of the matter

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of fact, as I think may keep wary men from hesitations about it. For, what is commonly reported concerning the *Divers*, is (as has above been intimated) grounded but upon their own Relations and answers, perhaps amplified or procur'd by leading Questions from persons, who are generally either slaves or ignorant men, taken from the less sober part of the illiterate vulgar, and prepossest with the common opinion of the non-gravitation of water in its own place; and consequently are not like to make over-accurate observations, but prone to refer the inconvenient alterations, they feel, to any other cause than the pressure of the water, which they are taught to be none at all. If observations about Diving were made by Philosophers and Mathematicians, or, at least, intelligent men, who would mind more the bringing up out of the

the Sea instructive observations than shipwrack'd goods, we should perhaps have an account of what happens to men under water differing enough from the common reports.

You will in one of the following *Papers* find mention of a Learned Physician of my acquaintance, that, upon his diving leisurely, perceived a constriction to be made of his *Thorax* by the action of the surrounding Sea-water.

A Spanish Prelate, that liv'd long in *America*, speaking of the deplorable condition of those wretched *Indians* that were employed by their inhumane Masters about the fishing for Pearls, gives us this account of them: * *It is impossible that men should be able to live any long season under the water without taking breath, the continual cold piercing them; and so they dye com-*

* See *Purch. Tom.* IV. *Lib.* 8. *p.* 1587.

commonly parbreaking of blood at the mouth, and of the bloody flux caused by the ſtomach. Their hairy which are by nature cole-black, alter and become afterwards a brunded ruſſet, like to the hairs of Sea-wolves, &c.

And a General of the *Engliſh* in the *Eaſt-Indies*, being by them employed on an Embaſſy to the Emperour of *Japan*, has this paſſage concerning ſome female Divers that he met with in his voyage: * *All along this coaſt (ſo up to Ozaca, we found women Divers, that liv'd with their houſhold and family in boats upon the water, as in Holland they do the like. Theſe women would catch fiſh by Diving, which by net & line they miſs'd, and that in eight fathom depth. Their eyes by continually diving grew as red as blood, whereby you may know a diving Woman from all other Women.* I know, it may be ſaid, that theſe diſ-

* *Purch. Tom.* I. *Lib.* 4. *C.* 1.

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eaſes may proceed from the coldneſs and moiſture or other qualities of the Sea; nor would I confidently reject ſuch a ſurmiſe: But it may alſo be poſſible, that the compreſſion, they ſuffer'd under water, might have at leaſt a ſhare in the production of theſe ill effects. For how are we yet certain, that the preſſure of the water againſt their bodies, though it does not manifeſtly diſlocate any ſolid or firm part, but only ſomewhat preſs inwards, as in the above mentioned Tadpole the outward skin and the fibres, (both which will eaſily yield a little way without being painfully ſtretch'd,) may not, by ſtraitning the Veſſels, and otherwiſe inconveniently, alter the circulation of the blood and the motion of the humors, ſpirits, and other fluid parts of the body? And I am not ſure, that much of the cold, that *Divers* are wont

wont to complain of, when under water, may not be a diſaffection produc'd in the nervous and membranous parts, occaſioned by the compreſſion of the ambient water, there being divers things, and preſſure among others, beſides actual cold, that will make men complain of being cold; and in our caſe this ſenſation may be excited or aſſiſted by the hindering of the uſual perſpiration at the conſtipated pores of the skin. And it ſeems not impoſſible, that one, not ſo ignorant and heedleſs as *Divers* are wont to be, may refer a new ſenſation, that really proceeds from preſſure, to other Cauſes; ſince Learned and Intelligent men, when prepoſſeſt (as theſe Common *Divers* uſually are) with the vulgar opinion about the Non-gravitation of Water and Air in their natural places, do

do almost always refer * an experiment of my Engine to *Suction*, which is indeed the effect of the pressure of the Ambient, (as I have † elsewhere clearly shewn,) and affirm, that the pulp of the finger or hand is drawn up into a hollow Pipe, into which it is indeed thrust by the weight of the Ambient air. But all these things I have mentioned, not as if I laid any great weight upon each of them, but to let you see, that 'twas not altogether without cause, that I complain'd of the incompetency of the History of what Divers feel under water; especially at great depths, where this want of information may be more considerable: For, as far as I have yet learnt by perusing Voyages and enquiring

* The reason of which experiment may be gathered from the 4th. Chapter of the Author's long since publish'd *Defence against Linus.*

† In a Paradox about *Suction.*

quiring of Travellers of my acquaintance, the places, where they are wont to dive for Pearl, are but moderately deep, and indeed ſhallow in compariſon of the great depths of the Sea; ſo that if we were furniſhed with as many Relations of theſe profound places, as we have of the others, poſſibly the accounts would be different enough to render doubtful or to correct the received opinions about the conditions of *Divers* at the bottom of the Sea. For, I remember that a credible eye-witneſs, who, (if I miſtake not) was the Intelligent *Oviedo*, ſpeaking of the Pearl-fiſhing on the American Iſland of *Cubagna*, has among many other notable obſervations ſuch a paſſage as this; *But whereas the place is very deep, a man cannot naturally reſt at the bottom by reaſon of the abundance of aery ſubſtance, which is in him, as I have oftentimes proved.*

proved. For although he may by violence and force descend to the bottom, yet are his feet lifted up again, so that he can continue no time there. And therefore where the Sea is very deep, these Indian Fishers use to tye two great stones about them with a coard, on each side one, by the weight whereof they descend to the bottom, and remain there until them listeth to rise again, at which time they unloose the stones and rise up at their pleasure.

And now to come closer to the explication of our difficult *Problem*; there yet occurrs to me nothing more likely in order to it, than what I have already mentioned in the Paper you will meet with about the *Differing pressures*, &c. And therefore it shall here suffice me to enlarge, and by further Considerations and Experiments confirm, what is there more summarily discoursed; namely, That the Phæno-

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menon may depend (chiefly) upon theſe two things, the *uniform preſſure* of the fluid Ambient, and the *robuſt texture* of a humane body expos'd to this Preſſure.

In one of the following * *Papers*, you will find examples of the great preſſure that may be ſuſtain'd unharm'd by ſuch frail bodies as Eggs and thin Glaſſes, that one would expect ſhould be broken in pieces thereby, provided the preſſure be exerciſed by the intervention of an Ambient liquor; as water. And by the account elſewhere refer'd to, of the *Tadpole*, it ſeems highly probable, that even that tender *animal*, when it ſeem'd by ſome ſmall diminution of the bulk to be every way a little compreſs'd inwards, was put to no conſiderable (or perhaps to any ſenſible) pain or inconvenience, ſince it ſeem'd

* *New Experiments about the differing Preſſure of heavy Solids and Fluids.*

to swim without any irregular motions, which would in likelihood have insued, if it had been much harm'd or incommodated. Which example, with those formerly pointed at, may teach us, that there may be a vast difference betwixt the resistance that a body can make when compress'd immediately by Solid bodies, & when in the compression every way ambient Fluids intervene. Which you will the less admire, if you consider, that by reason of the grosness, hardness, or rigidness of visible Solid bodies the pressure can never be made every where so equally as by the parts of Liquors, whose smalness, which renders them singly invisible, fits them to accommodate themselves far more closely and conveniently to all the superficial parts of the body immers'd in them, and to have the force of the compressing body more uniformly distri-

diſtributed to them. But becauſe the Inſtances referr'd to, are taken from bodies ſurrounded with water, I will take two or three about the reſiſtance of bodies to violently compreſs'd Air; *partly*, becauſe thoſe made in our Engine are wont to be perform'd with Air (not condens'd, but) rarified or expanded beyond its uſual conſiſtence; and *partly*, becauſe it will not be deny'd, that the corpuſcles of Air may be really compreſt or thruſt againſt one another, ſince 'tis clear, that they may be crouded into far leſs room, than they poſſeſs'd before, and bear ſo ſtrongly againſt the Glaſſes that impriſon them, as not ſeldom, if too much compreſs'd, to burſt them in pieces.

Conſider then, that among bodies not fluid the Swims of ſmaller fiſhes are likely to be judged none of the moſt able to reſiſt compreſſion, ſince

they conſiſt of bladders ſo thin and delicate, that a piece of fine *Venice*-Paper is very thick in compariſon, and that they contain nothing in them but ſoft Air not-compreſs'd by any outward force. I cauſed one of theſe bladders of above an inch in length and proportionably great, to be taken out of a *Roach*, and anointed it with Oyl to keep it ſupple, and preſerve it from being pierced or ſoftened by the water; and having by a weight of Lead, faſtend to the neck of it, let it down to the bottom of a hollow Cylindrical tube, ſeal'd at one end, and made purpoſely large, and about 56 inches long, for ſome Hydroſtatical Experiments; we could not perceive, that by the weight of all the incumbent water it was manifeſtly compreſs'd, or that it did diſcover the leaſt wrinkle or other depreſſion of the very thin membrane, though

though ſtuffed but with Air. And this tryal was made more than once with the ſame ſucceſs; and yet, that this proceeded rather from the robuſtneſs of the bladder, that was able to reſiſt the weight of a taller pillar of water, than from the Non-gravitation of water in the upper part of the tube on that in the lower, we ſhew'd, by preſently letting down ſuch a Mercurial-Gage as is deſcrib'd, & often mentioned in the Continuation of our *New Experiments*. For letting down this by a ſtring to the bottom of a tube, the weight of the incumbent water forced up ſome of the Mercury out of the open leg of the *Syphon* into the ſeal'd one, and conſequently compreſs'd the air included there, which though it were not very much, yet it was very manifeſt. For the uncompreſs'd Air being 3 inches and ½ in length, we judg'd it at the

bottom of the tube about $\frac{3}{4}$ by the intruſion of the Mercury that was impell'd up; and to ſatisfie my ſelf and others, that, if the incumbent water had been heavy enough, it would have viſibly depreſs'd the bladder in ſpite of any *Principium Hylarchicum*, ſince I could not have a tube long enough, the bladder was ſunk into a Chryſtal-Glaſs that had a long and Cylindrical neck, and was ſo well ſtuffed with a ſtopple that was Cylindrical too, that 'twas very difficult for any thing to get out betwixt it and the orifice of the Glaſs; then, a competent Quantity of air being left above the water, the ſtopple was warily and by degrees thruſt down, and ſo, leſſening the capacity of the Glaſs, compreſs'd the air that was next it, and, by the intervention of that, the water that was under it. And though there did not upon a ſlight compreſ-

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ſion of the outward air appear any ſenſible operation upon the *bladder*, that was at the bottom of the water; yet, upon a farther intruſion of the ſtopple the preſſure being encreas'd, the immers'd bladder diſcover'd not only one but two conſiderably deep wrinkles, which preſently diſappear'd upon the drawing up of the ſtopple. Upon whoſe being thruſt in again, depreſſions were again to be ſeen on the Swim. And we having been careful to conveigh into the ſame Glaſs ſuch a Mercurial Gage as has been lately ſpoken of, we eſtimated by the condenſation of the air in the ſeal'd leg of that Gage, that the *bladder* had been expos'd to a preſſure, that might be equivalent to that of a pillar of about 40 foot of water.

This I hope will leſſen the wonder, that Bodies of ſo firm a texture as thoſe of *luſty men*, ſhould ſupport

the preſſure of the water at ſuch depths, as *Divers* are wont to ſtay at; ſince we ſee, what reſiſtance can be made by ſo exceeding thin and delicate a membrane ſtuff'd only with air, in compariſon of the ſtrong membrans and fibres of a man, ſtuff'd beſides Air with more firm parts. I will not here urge, that great weights may be ſuſtain'd in the Air by ſuch *tendons* (or cords of fibres,) and by other fibres, as it were, interwoven into membrans, in compariſon of what an ordinary man would expect: But I ſhall invite you to conſider with me, that not only upon the account of the ſtable parts of the humane Body, but of the Spirits too, it may reſiſt very violent preſſures (and ſuch as perhaps have not yet been conſidered) of a fluid Body, not only without any manifeſt contuſion or diſlocation of parts, but without

without any ſenſe of pain; which I ſuppoſe you will grant me, if, conſidering what great effects Guſts of Wind have upon Dores, Trees, nay Maſts of Ships, blowing them down, nay breaking them; and that yet a man without being extraordinary ſtrong will ſtand againſt the impetuoſity of ſuch a ſtrong Wind, and walk directly againſt it by vertue of the vigour of his muſcles and ſpirits, without being thrown down or bruis'd by ſo violent a Current of Air as beats upon him, but without ſo much as complaining that he feels any pain; and this, though the Wind that beats againſt him, however it be a fluid Body, yet becauſe it acts as a ſtream, does not uniformly compreſs him, but invade only the fore-part of his Body. Likewiſe, in the lifting up heavy weights by Porters, Carriers and other luſty men, we may ſee the

the ſlender tendons of the hands loaded with 100 or 150, or perhaps a far greater number of pounds, without having their fibres ſo far compreſs'd or ſtretch'd as to make the lifters complain of pain, though ſometimes they may of difficulty. So that, (as I could, if it were needful, confirm by other Inſtances) a humane Body is an Engine of a much firmer ſtructure than Scholars are wont to take notice of. And here let me add, that I doubt, whether, if the ſtructure of a man were not conſiderably (though not perhaps equally) firm, he would, eſpecially in a deep Sea, be able to bear the preſſure of the water, though not immediately applyed, without pain. For (to give you one Reaſon more of my not acquieſcing in vulgar reports about Diving,) having ſeveral times convers'd with a man, apt enough both

both to enquire and obſerve, who got his living by taking up Ship-wrack'd goods, he anſwer'd me, when I ask'd him whether he felt any peculiar preſſure againſt the Drums of his Ears, which are membranes not ſo well back'd as thoſe of other parts; that when he ſtai'd at a conſiderable depth, as 10 or 12 fathoms, under the ſurface of the Sea, he felt a great pain in both his ears, which often put him to ſhifts to leſſen it; which by his manner of deſcribing it I concluded was from the incompetent reſiſtance of the Air, which he acknowledg'd to me he found by manifeſt tokens to be notably compreſs'd by the Superior water. Which Relation from ſuch a perſon does not only confirm our explication, but likewiſe warrant us to doubt, whether the Common Reports that are made concerning Divers be fit to be re-

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ly'd on, without farther Examen and obſervation.

In the mean time I ſhall add two or three Experiments more to confirm the reſiſtance, that Animals may make to a great preſſure, when exercis'd by the mediation of a fluid Body. And I the rather gave you an account of this way of making tryals, becauſe it may be alſo helpful to diſcover the reſiſtances of inanimate Bodies, whoſe Shape and Conſiſtence we may chooſe and vary (almoſt at pleaſure) to the preſſure of (totally or in great part) ambient fluids. And if I had been furniſhed with a tube wide enough, and a quantity of Mercury great enough, I might by the way have ſhewn you, that, whatever the Learned *Doctor More* is pleaſed to ſuppoſe, that to *Butter* it ſelf even as conſiderable a preſſure may be ſo applyed as not to be able

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to make it yield thereunto. For on this occaſion I ſhall adde, that I well remember, that, among other tryals to the ſame purpoſe, I cauſed a piece of freſh Butter, about the bigneſs of a ſmall Hen-Egg, to be brought to an irregular ſhape, that, if the compreſſion were ſuch as many would expect, the long corners or ſolid angles being at leaſt flatted, the Butter might be reduc'd into a more capacious figure and leſs remote from roundneſs. But though having put this lump of Butter into a Bladder, almoſt full of fair water, we proceeded, both in the ſame braſs Cylinder, and much after the ſame manner that I employed about the Egg mentioned in the Fourth Experiment of the *Tract of the Differing preſſure of heavy Solids and Fluids*; yet I found, that after the plugg had been loaded with a weight of Lead of above 50 pound, neither

neither I, nor the Operator, perceived, the irregular figure of the Butter to be altered. Nor was this the only tryal of this kind I made with the like ſucceſs upon Butter, though I dare not charge my memory with the Circumſtances; and therefore I ſhall without delay proceed to what I was about to recite concerning the Reſiſtance of *Animals*.

We took then a common *Fleſh-flie*, neither of the biggeſt ſort of all, nor of the leaſt, but of a middle ſize, and having put it into the ſhorter leg of a bent Glaſs, which we caus'd to be Hermetically ſeal'd at the end, there was put in as much Mercury as fill'd that leg and a part of the other, leaving little more than an inch of Air between the Quick-ſilver and the ſeal'd end, that there might be room both for the Fly and the Condenſation of the Air, and then with a little Rammer,

Rammer, fitted for the purpoſe, we caus'd the Mercury in the open leg to be thruſt againſt that in the ſeal'd leg, which thereupon did neceſſarily croud the Air near the Fly into leſs room; ſo that, by our gueſs, it was condenſed into about a third part of the ſpace, which it poſſeſs'd before, and which it regain'd, when the Rammer was withdrawn: And though this were done more than once, yet not only the Fly was thereby not kill'd, but not ſo much, that appear'd, as ſenſibly hurt, and I perceiv'd her, whilſt ſhe was pent up, to move her legs and to rub them one againſt the other, as 'tis uſual with that ſort of Inſects to do of their own accord in the free Air. Nor did I queſtion but that, if the Glaſs had not been inconveniently ſhap'd to admit the Rammer farther into it, the Fly would have ſupported a far greater Preſſure.

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Another Experiment to the ſame purpoſe we try'd with Water inſtead of Mercury; but, whereas this laſt named liquor could neither wet nor drown our Fly, (for which reaſon I chiefly made choice of it,) the other did firſt wet its wings, and ſoon after by a miſchance drown it. But firſt we had an opportunity to compreſs the Air into a third, if not into a fourth part of its former dimenſions, and yet the Fly continued to move divers of her parts and eſpecially her legs very vigorouſly, as if nothing troubled her but her being, as it were, glu'd to the inſide of the Glaſs by part of her wetted wings. And this I hope will keep the Reſiſtance of *Divers* to the Ambient water from ſeeming incredible; ſince ſuch Flyes were able to reſiſt, and (for ought appear'd) without harm or pain, the preſſure of the crouded particles of the

the Air; though we gueſs'd this to have been as much compreſs'd by the force of the Rammer, as it would have been by a Cylinder of water of 50 or between 50 and 60 foot high. By which alſo we may be help'd to conceive, how great a difference there is, whether the ſame preſſure be exercis'd by a ſolid or by a fluid Body. For, according to our eſtimate, the preſſure againſt the Body of the Fly was as great as if a ſlender pillar of Marble, having the Fly for its Baſe, and 18 or 20 foot in height, had lean'd upon the little *Animal*; which I preſume you will eaſily think was more than enough to cruſh her to Death.

But becauſe, though the fore-going tryals are not like to be rejected by the skilful, yet they require a ſomewhat dextrous and nimble Experimenter, and leave ſomething to

his eſtimate, I will ſubjoyn an Experiment more eaſie to be made, and wherein the weight may be determined by Meaſure rather than Conjecture, being made to be perpendicularly incumbent on the Fly or *other Animal.* For the Experiment may be as well made on *other* Inſects, as Worms, though ſome that I had provided chanc'd to miſcarry before they came to be uſed.

We took then ſome ordinary black Flies (ſuch as uſe to haunt Butchers ſtalls in warm ſeaſons,) of a middle ſize, (the length of the Body and Head of one *Animal*, which for trials ſake we meaſured, being about three eights of an inch,) and having placed one of them with the head upwards, that there was ſome diſtance left betwixt her and the ſealed end of the Glaſs-tube 9 or 10 inches long; we poured in Quick-ſilver very ſlowly and

and cautiouſly, leſt the force of ſo heavy a body, acquired by the acceleration of its deſcent, ſhould more than the meer weight it ſelf of the liquor oppreſs the Fly. To this effect ſtooping the Glaſs very much towards the *Horizon* and letting the Mercury paſs into the tube through a Funnel, whoſe lower part was very ſlender, that it might come down but by little and little, we at length got in as much Mercury as the tube would receive, and then holding it upright, we watched, whether the Fly would make any motions; and finding, that ſhe did manifeſtly ſtir notwithſtanding the incumbent Mercury, we meaſur'd the height of the Mercurial pillar, reaching from the middle of her body to the top of the liquor, and found it to be about eight inches, and the Quickſilver being poured out, the Fly appear'd to be ſo lively and vi-

gorous, that I doubted not, but if we had had a longer Glaſs, the Experiment had been much more conſiderable. But when afterwards I was able to procure a better tube, the ſeaſon of Flyes being almoſt quite paſt, I could ſcarce get any, and thoſe not brisk, as they are wont to be in Summer. But however we repeated the Experiment with one of the beſt we could take of the above-mentioned ſize, and ordering the matter ſo, that the Mercury incumbent on her, (for there was ſome beneath her,) appeared to be of a greater height than the formerly imployed tube was of, we ſaw her move one or other of her little leggs divers times, though the tube were held upright; and therefore meaſuring the height of the Mercury above her, we found it to amount to 16 inches and better, and then freeing her from this preſſure, we obſerved,

ved, that ſhe immediately found her leggs again, and moved up and down briskly enough; but when ſhe was loaden with 23 or 24 inches of the ſame Quickſilver (though the liquor were ſoon after poured out) ſhe gave no ſigns of life, which I ſuſpected might happen, not ſo much from her having been oppreſt by the greatneſs of her weight, as from the great care of the Operator to let down the Mercury very obliquely and warily upon her. And this I was the rather confirm'd in, becauſe having got an other Fly of about the ſame bigneſs, though when ſhe was at the bottom of the Quickſilver, ſhe ſeemed ſo compreſt as not to have any motion we could take notice of, yet, upon her being taken out of the Glaſs, ſhe preſently appeared to be alive by walking about and beginning to diſplay her wings, though the pillar of Mer-

cury, that had leaned upon her, amounted to above 27 inches. And I preſume, the ſucceſs would have been much more conſiderable, if the Experiment had been tryed in the Summer, when theſe Creatures are brisk and lively, and not as it was in the Winter; beſides that probably theſe little Animals were hurt or weaken'd by the violence that would ſcarce fail to be us'd in catching them, and putting them into ſuch a place and poſture in the Glaſs as was required; the actual coldneſs of the Quickſilver perhaps alſo making them ſomewhat torpid, whilſt it touched them ſo many ways. And it muſt not be here omitted, that a Fly, that ſeemed but about half ſo big as one of thoſe hitherto mentioned, being well placed, with ſome Mercury under it, in a Glaſs-pipe held upright, ſuſtained a Mercurial pillar of ſome-

what

what above 25 inches; and though ſhe was not obſerved to move under ſo great a weight, yet when once it was taken off, ſhe did not appear hurt, much leſs cruſh'd to Death by it, and probably would have eſcap'd under a much greater weight, if the tube, which was too large, had not already imployed all the ſtock of Mercury we then had at hand. But I do preſume, that what we did try will be available to our purpoſe, ſince we ſee clearly, that ſo ſmall an *Animal* as a Fly may ſurvive ſo great a preſſure, and that ſhe could not only live, but was able to move ſuch long and ſlender Bodies as her leggs, when ſhe was preſſed againſt by above 16 inches of Mercury, and (conſequently) by a weight equivalent to a pillar of water of above 18 foot and a half, which being above 590 times her own length, and (according to

the eſtimate our meaſure ſuggeſted) many times more her own height; ſo that a *Diver*, 6 foot tall, (which is ſomewhat more than an ordinary mans ſtature,) to have as many times his height of water above him, as our Fly might have had and yet have moved under it, muſt dive (at leaſt in freſh water,) to near a hundred fathom, which is a far greater depth (perhaps by 5 or 6 times) than, for ought I could learn by inquiry, the Divers either for Coral or Pearl are wont to deſcend.

And now, Sir, having tender'd you the likelieſt conjectures that occurr'd to me about the ſolution of this difficult Problem; I ſhall return to Doctor *More*, and conſider the objection, he frames from the ſuppoſed inſolubleneſs of it. And on this occaſion I ſhall have two or three things to repreſent to you.

The

The *firſt* is, that there would be much more weight in what he objects, if our Aſſertion of the gravitation of water in water were, like the *Principium Hylarchicum*, a meer Hypotheſis advanc'd, without any clear poſitive proof, whereas our Doctrine is not only elſewhere directly proved, by particular Experiments, but by the very controverted one of the *Tadpole*; to elude whoſe force ſo Ingenious a perſon is fain to flye to a Principle, that, (to ſay *here* no more,) is not Phyſical. And from this firſt of the things I lately mentioned I ſhall haſten to the *ſecond*, becauſe it will require to be longer inſiſted on.

I ſhall then further repreſent that whatever power he is pleas'd to ſuppoſe at the bottom of the Sea to ſuſpend the impreſſion of the incumbent Water, I think, that ſuppoſition ought to give place, if not to our former

mer Ratiocinations, yet to experience it ſelf, which ſhews there really is a great preſſure exercis'd by the Water at the bottom of the Sea. I * Sir R. M. remember, that a friend of the Learned * Doctors and mine, who is ſo eminent a *Virtuoſo* as to have been often Preſident of the *Royal Society*, related a while ſince to me, that a Mathematical friend of his, whom he nam'd, having had an opportunity to try an Experiment, I have in vain endeavoured to get tryed for me, had the Curioſity to let down in a deep Sea a Pewter-bottle with weight enough to ſink it, that he might try, whether any ſweet Water would ſtrain in at the orifice or any other part; but when he had pull'd it up again, he was much ſurpriz'd to find the ſides of his Pewter-bottle very much compreſs'd, and, as 'twere, ſqueez'd inward by the Water. I

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alſo not long ſince inquir'd of an obſerving Acquaintance of mine, that has a conſiderable eſtate in *America*, whether he had not try'd to cool his drink, when he ſail'd through the *Torrid Zone*, by letting down the bottles to a great depth into the Sea, and, if he did, in what Condition he found them when they were drawn up again. To which he anſwer'd, that he had ſeveral times employ'd that Expedient for the Refrigeration of his Drinks, but was at firſt amaz'd to find the Corks, with which the ſtrong ſtone-bottles had been well ſtopt before, ſo forcibly and ſo far thruſt in, that they could ſcarce have been ſo violently beaten in with a Hammer, and 'twas ſcarce poſſible to get them out. And an other Ingenious Perſon, that practiſes Phyſick in the *Indies*, having the like Queſtion put to him, anſwer'd me, that

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he had ſome while ſince had the Curioſity to try in a very deep part of the Sea, whether any freſh Water would ſtrain into Stone-bottles through a thick Cork ſtrongly ſtopt in, and having let it down with a convenient weight to 100 fathom, was much diſappointed, when he drew it up, by finding that the preſſure of the Water at ſo vaſt a depth had quite thruſt down the Cork into the Cavity of the bottle (which elſe perhaps would have been cruſhed to pieces;) an effect which he would ſcarce have expected from the ſtroaks of a Mallet. And if to all this it be objected, that 'twas not the preſſure, but the coldneſs of the Water that did the recited feats by condenſing the included Air, and obliging Nature to do the reſt for fear of a *Vacuum*; I will not lanch into the Controverſie, whether Nature do any thing

ob fugam Vacui, but only anſwer, that I cannot find by the Relations of the *Divers* or otherwiſe, that 'tis ever ſo cold at the bottom of the Sea, as 'tis frequently above ground in Winter, when great Fiſhes are commonly ſaid to return to the deep parts of the Sea for warmth, and yet in the ſharpeſt Winters I never obſerv'd Corks to be driven in by the cold of the Ambient; nay, I purpoſely tryed with a Frigorifick mixture, that very intenſe degrees of cold, ſuch as would quickly freez many Liquors, would not occaſion the breaking of thin bubbles of Glaſs purpoſely blown at the flame of a Lamp and hermetically ſealed.

And to ſhew *ad oculum* (as they ſpeak) that Water may preſs more and more, as it grows deeper, againſt the ſtopple of a Bottle, though the Veſſel be inverted, I will ſubjoyn this

this Experiment. Becauſe we have no Water hereabouts that is near deep enough to force in a Cork, as the Sea-water did in the above recited tryals, I thought of a way of ſo cloſing the Glaſs-veſſel, as that the ſtopple ſhould keep aſunder the Air in the Veſſel and the outward Water, and hinder all immediate intercourſe between them, and alſo make ſome reſiſtance againſt the preſſure of the external Water, and yet be capable of freely moving up and down, and ſo be a good *Succedaneum* to a ſolid ſtopple. Taking then a Glaſs-Vial, furniſhed with a (ſomewhat long) Cylindrical neck, whoſe Cavity was large in proportion to the reſt of the Veſſel, we put into it as much Quickſilver as would in the neck make a ſhort Mercurial Pillar of between half an inch and an inch; then, a piece of very fine Bladder, dipp'd in Oil, was ſo tyed over

over the orifice of the Glaſs, that no Mercury could fall down or get out, nor Water get in at the orifice, and yet the Bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the Cavity of the Vial, or depreſs'd by the weight of the Mercury. This little inſtrument, firſt furniſhed with a weight of Lead to ſink it, being inverted, the Mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple, and yet with its lower part depreſs'd the Bladder beneath the Horizontal Plane, that might be conceiv'd to paſs by the orifice; then the Glaſs, being a while kept in the Water, (that the included Air might be brought to the Temperature of the ſurrounding Liquor,) and by a ſtring let further down into the ſame Glaſs-veſſel fill'd to about two foot in height, the preſſure of the Liquor againſt the orifice

of

of the Vial did by degrees drive up the Bladder and the Mercurial ſtopple into the cavity of the Neck, as was manifeſt by the aſcenſion of the Quickſilver; and when the inſtrument was leiſurely drawn up again, the weight of this Mercury made it ſubſide and plump up the Bladder again as before. An Experiment a kin to this, and therefore fit to confirm it, I have deliver'd in another * Diſcourſe.

* See the Paradox about Suction.

And here I ſhall ſubjoyn what very opportunely occurr'd to me ſince the writing of the laſt page. Meeting caſually with an Ingenious Mechanician, (whom you will find I have * elſewhere mentioned) that deviſed a ſuit of cloaths and other accommodations, (wherein I once ſaw him let down into the Water,) by whoſe help and that of a boat

* In the *Tract of the Differing Preſſure of heavy Solids and Fluids.*

a boat he could (and did) continue there a great while at a considerable depth under water, and there work; I ask'd him afresh (to obtain fuller informations than formerly) whether he felt not the pressure of the water against his breast and belly, to which he answer'd me (more circumstantially than he had before) that when he was about 4 or 5 yards under water, though but in the River *Thames*, his breast and *abdomen* was so compress't, that there being hardly room enough left for the free motion of his Lungs he could scarce fetch his breath, and was necessitated to make them draw him quickly up, and that (among his later tryals to improve his Engine) having for remedy hereof, caused a kind of Armour for the Chest and back to be made of Copper, though the stiffness of the Metal defended

him from receiving any miſchief in thoſe parts, yet in the others, where only the Leather, though ſtrong, was interpoſed, when he came to the depth of about ſix fathom, though in freſh water, he found a great preſſure againſt his legs and armes and all the other parts againſt which the water was able to thruſt the Leathern ſuit inwards. And this preſſure being found by him, as he told me, pretty equal (againſt all the expoſed parts, for from the other, which were more yielding and obnoxious, the Armour kept it off,) he received no Miſchief from it, nor yet *much* Incommodity (and *ſome* he might expect from the ſtiffneſs and unequal yielding of the Leather;) ſo that he could ſtay under water, though not ſtill at ſo great a depth, about 2 hours or longer. And upon the whole matter he anſwered me, that he was well

well ſatisfied by his tryals, that the ambient water endeavoured to preſs him & his Diving ſuit every way inwards. Whether the coldneſs of the water had any intereſt in this *Phænomenon*, I particularly enquired of the Engineer; but he replyed, that by reaſon of the tightneſs of his Diving ſuit or inſtrument, the warm ſteams of his body that were pent in, and other concurring circumſtances kept him from feeling any cold, and made him ſometimes feel a greater Heat than he wiſhed. He has promiſed me before it be very long to make for me a tryal or two that I propounded to him, from whoſe ſucceſs, if he can but reduce them to Experiment, I hope to be able to preſent you a farther Confirmation of our Hypotheſis. In the mean time, the things already recited, together with the preceed-

ing Experiments, may well ſuffice for our preſent purpoſe. For, by what hath been ſaid it appears, that Water does actually preſs againſt bodies, whether ſpecifically lighter or heavier than it ſelf, placed under water, and that this preſſure increaſes with the height of the water above the immerſed Bodies. And this being ſo, it is not more neceſſary for me than for men of other Opinions to give a clear reaſon why *Divers* can reſiſt ſo great a preſſure of the incumbent water. And the preſſure of the water in our recited Experiment having manifeſt effects upon Inanimate bodies, which are not capable of prepoſſeſſions or giving us partial informations, will have much more weight with unprejudiced perſons, than the ſuſpicions and ſometimes diſagreeing accounts of ignorant *Divers*, whom preju-

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dicate opinions may much ſway, and whoſe very ſenſations, as thoſe of other vulgar men, may be influenced by Prediſpoſitions and ſo many other Circumſtances, that they may eaſily give occaſion to miſtakes. I know, that Learned men, that never were converſant in Hydroſtaticks, are wont to think it very difficult, if not impoſſible, to conceive, how ſo weak a thing, as they fancy an *Animal* to be, ſhould avoid the being oppreſs'd or ſo much as harmed by ſo great a weight of Water. But they that ſhall attentively conſider what has been offer'd towards the removal of this difficulty, and remember, how little they would have believed, that there is ſo great a difference, as we have by the Tadpole, the Fly and other inſtances, ſhewn there really is between the preſſure of Solid and of Fluid bo-

dies, will, I presume, be apt to think it fit, that, if for want of a sufficient History of matters of fact any scruple remain about the Solution we have offer'd from the nature of the Uniform pressure of Fluids, and the Firm structure of the Humane body; we should, to remove those remaining scruples also, rather range about for other Physical helps to solve more compleatly the Problem, about such a thing as Compression, which is an action purely Corporeal and Mechanical, than for want of a ready and compleat Solution to flye to the immediate interposition of an immaterial and intelligent yet Created Agent, to explain clearly whose manner of working would be a much more difficult Task, than the solution of the Phænomenon without it.

And now, Sir, having presented

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to you the Reflections I thought requiſite to write upon the Learned Doctors diſcourſes againſt my Hypotheſis and Explications, relating to the gravitation and preſſure of Fluids, I have little more to trouble you with in this Paper. For, though in the latter part of the 13th. Chapter the Doctor is pleaſed to ſpend divers pages in the Explication of divers of my Hydroſtatical Phænomena by the Agency of that incorporeal Director, that he calls *Principium Hylarchicum*; yet ſince theſe Explications of his are rather attempts to accommodate the *Phænomena* to the Hypotheſis, than objections directly levell'd againſt my Solutions, I ſhall altogether forbear to examine them; the main thing that I intended in this Paper, according to what I told you at the beginning, being to ſhew, that the

Arguments urg'd against the Mechanical solutions of the Experiments by me recited, do not evince any of them to be erroneous. And I have neither the design nor the leasure solicitously to examine the *Doctors Hylarchical Principle.* Of which I shall only say, that though he tells us, it is * *paratum ad movendum quoquoversum materiam pro data occasione*; yet since he also tells us, * *Quòd particulæ molis corporeæ sive stabilis sive fluidæ à Principio Hylarchico in unam aliquam partem omnes junctim urgeri possunt & premi, quamvis singulæ singulas in nullam partem premant, quodque pro magnitudine molis major minorve totius fit pressio*; and that the force by which it endeavours to keep the Elements in their true and natural Con-

* Page 175.

* Page 167.

Conſiſtence, though it be very great, is not invincible * : I ſee no need we have to flye to it, ſince ſuch Mechanical Affections of matter, as the Spring and Weight of the Air, the Gravity and Fluidity of the water and other Liquors, may ſuffice to produce and account for the Phænomena without recourſe to an Incorporeal Creature, which 'tis like the Peripateticks and divers other Philoſophers may think leſs qualified for the Province aſſign'd it, than their *fuga Vacui*, whereto they aſcribe an Unlimited power to execute its Functions. I leave it therefore to you, Sir, to judge which of the two ways, of explicating an Hydroſtatical Phænomenon, the Learned Doctors, or that which I have made uſe of, reliſhes moſt of the Naturaliſt. And I ſhall only tell you, that if I had been

* Pag. 167.

been with thoſe Jeſuites, that are ſaid to have preſented the firſt watch to the King of *China*, who took it to be a living Creature, I ſhould have thought I had fairly accounted for it, if, by the ſhape, ſize, motion, &c. of the Spring-wheels, balance and other parts of the watch I had ſhewn, that an Engine of ſuch a ſtructure would neceſſarily mark the hours, though I could not have brought an argument to convince the Chineſe-Monarch, that it was not endowed with Life. From which compariſon you will eaſily gather, that what I have thought my ſelf concern'd to doe in this place, was not to demonſtrate in general, that there can be no ſuch thing as the Learned Doctors *Principium Hylarchicum*, but only to intimate, that, whether there be or not, our Hydroſtaticks do not need it.

Nor

Nor do I think it neceſſary to the Doctors grand and laudable deſign, (wherein I heartily wiſh him much ſucceſs) of proving the exiſtence of an Incorporeal ſubſtance. For as I think, Truth ought to be pleaded for only by Truth; ſo I take that, which the Doctor contends for, to be evincible in the righteſt way of proceeding by a perſon of far leſs learning than He, without introducing any precarious Principle; eſpecially experience having ſhewn, that the generality of Heathen Philoſophers were convinc'd of the being of a divine Architect of the World, by the contemplation of ſo vaſt and admirably contriv'd a Fabrick, wherein yet taking no notice of an immaterial *Principium Hylarchicum*, they believed things to be managed in a meer Phyſical way according to

to the General Laws setled among things Corporeal, acting upon one another. And after this I have no thing more to say, but that I would not have any thing that I have said misconstrued to the Learned Doctors prejudice. For 'tis not necessary, that a great Scholar should be a good Hydrostatician. And a few hallucinations about a subject, to which the greatest Clerks have been generally such strangers, may warrant us to dissent from his opinion, without obliging us to be enemies to his Reputation. And therefore if you have found any thing in this Paper inconsistent with a just tenderness of that, you have not only my consent, but my desire to alter it, as an Expression, that doth not well comply with my Intentions of not appearing any farther his Ad-

Adverſary in our Debate, than the deſire of ſhewing my ſelf a Friend to the Truth I was to defend, ſhould exact of,

SIR,

Your, &c.

An Hydroſtatical

LETTER,

Written *Feb.* 13. 167$\frac{1}{2}$.

Containing a Dilucidation of an Experiment of the Honourable Author of theſe Tracts about a Way of *Weighing Water in Water*, upon the occaſion of ſome Exceptions made to it by Mr. *George Sinclaer.* *

* In his *Hydroſtaticks*, printed at *Edenburg* 1672. p. 146. ſſ.

An Hydrostatical

LETTER

TO THE READER.

WHen this Diſcourſe was juſt finiſhing in the Preſs, there came to the Publiſher's hands a dilucidation of an Experiment of the Honourable Author of theſe Tracts, about a Contrivance of his for Eſtimating the Weight of Water in Water, formerly publiſht in Numb. 50. *of the* Philoſoph. Tranſactions, *and by the following Diſcourſe clear'd from the exceptions to be met with in* Mr.

George Sinclair's *Book*, *entitl'd* The Hydrostaticks, &c. *printed at* Edenburg, 1672. *Which Dilucidation, because of the Affinity of the Subject, was thought fit to be here annext.*

An

An Hydroſtatical

LETTER,

Written *Feb.* 13. 167½.

Containing a Dilucidation of an Experiment of the Honourable Author of theſe Tracts about a Way of *Weighing Water in Water*, upon the occaſion of ſome Exceptions made to it by Mr. *George Sinclair*. *

* In his *Hydroſtaticks*, printed at *Edenburg* 1672. *p.* 146. *ſſ.*

SIR,

CAlling this night in *Pauls* Church-yard for the Ingenious Mr. *Rays* Travels, that you yeſterday commended to me, I was alſo ſhewn a New Treatiſe, that I never ſaw before, of a Learned Gentleman, and haſtily running over the *Index*, found an Experiment of mine declared In-

ſufficient; and *though*, being hinder'd to make haſt home, it be ſo late, that far from having time to peruſe the book it ſelf, (which I tell you, that you may not now expect any Character of it from me,) I have been ſcarce able to read over, more than once, what directly concerns me in it; *yet* I ſhall adventure to ſay ſomething about it this night, for fear I ſhould not, in ſo buſie a time as this, be allowed to do it to morrow.

Whereas then the Learned Objector having recited my experiment about weighing Water in Water, as you were pleaſed to publiſh it in a book enriched with ſo many better things, the * *Philoſophical Tranſactions*, begins his animadverſion with ſaying that *herein is a great miſtake*. I ſhall not in that much oppoſe him: For poſſibly the Diſpute between us is not much

* Numb. 50.

much more than verbal. And becauſe my Experiment coming abroad by it ſelf, and ſuppoſing things that I had formerly proved, and publiſhed, but which were not expreſly referr'd to in it, I wonder not that my meaning ſhould not by all Readers be fully underſtood. And therefore, to explain my ſelf on this occaſion, give me leave both to repeat my Opinion, and to ſhew you, on what occaſion and how far I deſign'd to confirm it by this Experiment. My opinion then was, and ſtill is, that as water is a heavy fluid, ſo it does retain its Gravitation and power of preſſing; by which I mean a tendency downwards (whatever the cauſe of that gravity be,) whether it have under it a body either ſpecifically heavier or lighter than it ſelf or equiponderant to it. For I ſee not what ſhould deſtroy or

aboliſh this Gravity, though many things may hinder ſome effects of it. And therefore I ſuppoſe, that Water retains its Gravity not only in Air but in Water too, and in heavier liquors, and conſequently, by vertue of this, the liquor preſſes upon them; but if a ſurrounding fluid have, upon the ſcore of its ſpecifick Gravity, an equal or a ſtronger tendency downwards than water, it will, by vertue of that, be able to impel up this liquor or to keep it from actually deſcending: ſo that a portion of water, ſuppoſed to be included in a Veſſel of the ſame ſpecifick weight with water, this portion, I ſay, placed in a greater Quantity of the ſame water will neither riſe nor fall, as I have elſewhere ſhewn; but yet it retains its Gravity there, only this Gravity is kept from making it actually deſcend by the

the contrary action of the other water, whoſe ſpecifick Gravity is ſuppoſed equal; as when a juſt ballance is loaded with a pound weight in each of its ſcales, though neither of the weights actually deſcend, being hinder'd by its counterpoiſe, yet each retains its whole weight, and with it preſſes the ſcale it leans upon; ſo that our lately mentioned included portion of water does really *preſs* the ſubjacent water, though it does not actually *depreſs* it, or, (as perhaps a School-man would phraſe it) does *gravitate* on it but not *pregravitate*. Nor do I think, that the only way of judging, whether a body gravitates, is to obſerve, whether it *actually* deſcends, ſince in many caſes its Gravity may be proved by the Reſiſtance it makes to heavy bodies, which, if it were not one, would raiſe it: As may be

declar'd by what I juſt now noted about equal weights in a ballance: And for want of this diſtinction I have known even learned men, treating of Hydroſtatical things, miſtake both me and the Queſtion.

The next thing I had to tell you, is, that the Adverſaries, I had to deal with both in Print and in Diſcourſe, denyed, that in (ſtanding) Water, the upper parts did preſs or gravitate upon the lower; and though they could not but grant, that the whole weight of the Water did gravitate upon the bottom of the Veſſel; yet they would have the parts of it to do ſo *actione communi* (as they ſpeak) and fancied I know not what power of Nature to keep the homogeneous portions of Water, as well as other Elements, from preſſing one another, when it is in its proper place. Againſt this

this Opinion, (which I preſume my Learned Adverſary and I agree in oppoſing,) it was alledged, beſides other things, which I found many, otherwiſe good, Scholars were not fitted to underſtand, That if a Glaſs-vial or bottle, well ſtopt, were deeply immerſed under water, it would ſtrongly tend upwards; but if it were dextrouſly unſtop'd, when 'twas thus immers'd, ſo as the water could get in, abſtracting from or allowing for the weight of the Glaſs it ſelf, 'twould by the water, that crouds in and thruſts out the Air, be made ſtrongly to tend downwards and continue ſunk. But this not ſatisfying, becauſe 'twas pretended, that the reaſon of the empty bottles emerging when ſtopt was the poſitive Levity of the Air it was filled with, and the ſinking

of

of it, when unſtopt, was from the receſs of the ſame Air, that by the intruding Water was driven with large bubbles out of the bottle; I thought this evaſion might be obviated by contriving an Experiment, wherein the Water ſhould be plentifully and ſuddenly admitted into the Glaſs, and yet no Air expelled out of it, (which Circumſtance I therefore took notice of, where I ſay, *no bubble of Air appear'd to emerge or eſcape through the water,*) ſo that, if then the Glaſs that was kept up before ſhould fall to the bottom with a gravitation amounting to a conſiderable weight in reſpect of its capacity, the ſinking of it could not by them be aſcribed, as before, to the receſs of the Air, endowed, as they ſuppoſe, with poſitive Levity, but to the weight of

of the admitted Water, which, when thus weighed, would be inviron'd with Water of the ſame kind: And to ſhew, that this admitted Water might have a conſiderable weight notwithſtanding the place it was in, I imployed a pair of ſcales after the manner that is recited in the Experiment.

By what I have been diſcourſing, you may conceive, that, however my expreſſions diſagree with thoſe of my Adverſary, the diſtance of our opinions is not ſo wide as at firſt ſight it ſeems. For he allows as well as I, that the ſuperiour parts of Water do by their Gravity, (for I know not on what other ſcore they can do it,) preſs the inferiour. But this he would not have amount to this expreſſion, *that water weighs* or gravitates *in water*; whereas I ſcruple not to cloath my ſence in

in that expression, because I think, water does always exercise its gravity, though it does not always *pregravitate* or *actually* descend, being often (as I noted above) either impell'd up by an opposite and prepollent weight, or hinder'd from descending by the Resistance of other water that counterpoises it: so that, if he thinks, that in my Experiment I meant to propose a method of making Water descend in Water, and weigh it in that Liquor with a pair of scales, *just* as if I would weigh in the same Water a piece of Lead or a portion of Mercury, which are bodies much heavier *in specie* than Water, either he mistakes my intention, or I did not sufficiently declare it. But that which I designed to shew, and, for ought I can yet see, have shewn, was, that *by the help*

of

of an ordinary ballance, it may be made appear, that Water admitted into the Glaſs-bubble, I imployed, did make the Glaſs-bubble weigh ſo much heavier than it did before that Liquor enter'd into it; and that this new weight, that was manifeſted by the ballance, was not due, as my Adverſary ſuppoſed, to ſuch a receſs of the Air as I mention'd a while ago.

And now, Sir, It will be proper to take notice of ſome paſſages in the Objectors Diſcourſe, in order to dilucidate the ſubject of it. Whereas he ſayes (page the 149. & 150.) *Take a piece of wood, that is lighter* in ſpecie *than Water, and add weight to it by degrees, till it become of the ſame weight with Water; knit it with a ſtring to a ballance, and weigh it in Water, and you will find*

the

the whole weight ſupported by the water. I anſwer, that this does not at all overthrow my opinion, but agrees very well with it. For, ſuppoſe, the weight you add to the light wood be Lead, it cannot be ſaid, that the Metal loſes its native ponderoſity whilſt it reſts in the Water; and the reaſon, why it deſcends not, is, that it and the wood, it is joyn'd to, are hinder'd by the counterpoiſe of the Collateral Water, which by its preſſure would raiſe the ſurface of the Water, whereon the floating or ſwimming body leans, if it were not hinder'd by the weight of theſe incumbent Solids: And this Reſiſtance of theirs to the endeavour upwards of the Water, being exerciſed only upon the account of their Gravity, ſhews that they do in my ſence *gravitate* (though not *pregravitate.*) Again,

Again, if you pleaſe to conſider the caſe, put by the Objector (page the 151.) and caſt your eyes upon his Scheme, which, (ſuppoſing you to have his book) I ſhall for brevities ſake make uſe of at preſent; you will find him thus argue. * *Now, I ſay, 'tis ſix ounces of the weight (B) that makes this alteration, and turns the ſcales: For, if 12 ounces ſink the Glaſs below the Water, when it is full of Air and no Water in it, then ſurely ſix are ſufficient to ſink it when it is half full. And the reaſon is, becauſe there is a leſs* potentia *or force in 6 inches of Air by the one half to counterpoiſe a weight of 12 ounces, than in 12 inches of Air. Therefore this Air being reduc'd from 12 inches to ſix, it muſt take only 6 ounces to ſink it.*

* Vid. page 151.

To

To which I answer, that I know not yet, what, on this occasion, he means by a *potentia* or force in 6 inches of Air to counterpoise a weight of 12 ounces. For by the term *counterpoise*, where the Question is about Weighing, one would think he speaks of Weight; and yet Air, according to the vulgar Opinion, is positively light; according to us, though it have a gravity, yet in our ease that must amount to so little, that what Air the bubble needed to fill it, could not weigh at most above 4 or 5 grains, which therefore might safely be neglected. But, according to my opinion, the reason of the *Phænomenon* is clear enough without medling with the *Potentia* of the Air. For, if we conceive a horizontal Plane to divide the Water mentally, and pass by the

the bottom of the ſuſpended bubble; before the little ſtem be taken off, there is a far greater preſſure upon the other parts of that Plane than upon that which lies under the bubble, in regard they are preſt by the weight of the Collateral Water (*A*, *L*, *G*, *D*, *M*, *C*,) whereas the other is preſt only by the weight of a body very much lighter than its equal bulk of water: ſo that, to keep the bubble from being forcibly buoyed up, there was requiſite 18 ounces of Lead that make up the Plummet (*B*) to detain it under Water, and keep the beam of the ballance Horizontal; that when acceſs is given (at *C*) to the neighbouring Water, it is by the weight of the collaterally ſuperiour Water impell'd into the cavity of the bubble, where the Air, being much rarified

before, could not reſiſt its ingreſs, and thereupon 6 ounces of Water getting in, that part of the imaginary Plane, on which the bubble was incumbent, is preſt by a greater weight than formerly by 6 ounces, and conſequently there needs the like weight in the oppoſite ſcale of the ballance, to reduce the ſcale to an *Æquilibrium*. And if we ſuppoſe, with our Author, the Glaſs to be compleatly full of Water, and the counterpoiſe in the ſcale (*O*) to need 6 ounces more to make a new *Æquipondium*, the account of the *Phænomenon* will be the ſame, as, if you attentively conſider it, you will clearly perceive. And the reaſon, why the additional weight of 6 ounces is required, will be, that the upper half of the bubble, that before contained leſs than three or

four

four grains weight of the Air, being now fill'd with Water, amounted to ſix ounces more of Water than formerly, and ſo the counterpoiſe in the oppoſite ſcale (*O*) will need the weight of ſix ounces to make a new *Æquipondium*.

Congruouſly to this explication when the Examiner ſays,

* *Now I inquire whether theſe* 18 *ounces are the Æquipondium of the Water within the Glaſs or of the weight of the Lead* (*B*)*? 'Tis impoſſible they can counterpoiſe both, ſeeing the Water is now* 12, *and B* 18. *It muſt then either be the counterballance of the Water or the counterballance of the Lead. It cannot be the firſt, becauſe* 12 *cannot be in Æquipondio with* 18; *it muſt then be in the ſecond: Or if theſe* 18 *ounces in the ſcale* (*O*) *be* * page 132.

the counterpoiſe of the Water within the Glaſs, I inquire what ſuſtains the weight of the Lead (B)? The weight of it cannot be ſuſtained by the Water, becauſe 'tis a body naturally heavier than Water; it muſt therefore be ſuſtained by the ballance. I anſwer, that this ſpecious objection ſeems (for it is ſomewhat obſcurely worded) to be founded upon a miſtake of my meaning in the Queſtion. However, as to the Phænomenon it ſelf; according to my ſence the 18 ounces in the ſcale (*O*) are the counterpoiſe of the 18 ounces that hang from the oppoſite and æquidiſtant Scale and make up the Leaden Plummet (*B*,) (which anſwer I ſee not how our Author prevents.) But then you will ask, what counterpoiſes the Water in the bubble, which alone weighs 12 ounces? I anſwer,

anſwer, that 'tis the gravitation of the collateral Water, which preſſes the other parts of the lately mentioned imaginary Plane, as much as the Water in the bubble, the weight of the Glaſs being here not reckon'd by either of us; and the Water incumbent on the bubble does preſs that part of the Plane on which they lean; ſo that there being in all 30 ounces to be ſuſtained, the 18 of the Plummet and the 12 contained in the Glaſs, the Lead that hangs in the Water is counterpoiſed by 18 ounces in the ſcale, and the Water in the bubble by the preſſure of the Collateral Water.

But you will ſay, that it appears not, that the included Water preſſes at all, ſince it does not at all deſcend. To which I anſwer,

that as long as the Water was getting into the Cavity of the bubble, so long it did manifestly gravitate upon the subjacent Plane, and actually descend, raising the counterpoise in the scale: But when, by adding more weight to that counterpoise, things are brought to a new *Æquilibrium*, there is no reason why the gravitation of the VVater should again change the now regain'd *Æquipondium*. Suppose in the two scales of a ballance there were placed two equally capacious and equiponderant Vials, whereof one is *quite* full and the other *almost* full; 'tis evident, that the full Vessel will keep the scale it lean'd upon depress, and, if you gently pour in as much VVater into the unfill'd as the fill'd has more than it, the scale, that was formerly kept

kept rais'd, will be now depreſt, till the beam be brought to be horizontal; to which poſture when it is once brought, the *Æquilibrium* will continue: And yet it will not be ſaid, that though the added VVater, whilſt it was filling the Glaſs, depreſt the ſcale it belonged to, yet it loſt its weight, or, which in my ſence is all one, did not gravitate upon the Scale, when the ballance was come to an *Æquilibrium*, becauſe then this VVater did no longer depreſs it. And how much the VVater in our bubble does, notwithſtanding its immerſion, gravitate, would be viſible, if by ſuppoſition it were all annihilated, and no other ſuffer'd to ſupply its room. For, then the ſubjacent part of the imaginary Plane being much leſs preſt than immediately before, the

weight of the collaterally ſuperiour VVater would ſtrongly impell up the bubble, if it were not kept in its place by a proportionable addition of weight to the Plummet. Nor ſhould it ſeem a ſtrange thing that I ſhould ſay, that the 30 ounces, lately mentioned, ſhould be counterballanced partly by the weight in the oppoſite Scale, and partly by the VVater that fills the immers'd bubble, ſince this notion may be warranted even by the common practice of weighing heavy Solids Hydroſtatically. For if you would, *for inſtance*, weigh a lump of Copper of 9 pound in common VVater, the Metal, hanging by a Horſe-hair under VVater, will need, according to my elſewhere mention'd Experiments, either juſt or near about 8 pound in the oppo-

ſite

ſite Scale to keep the ballance horizontal, ſo that the whole 9 pound, that the lump weighed in the Air, is counterpoiſed partly by the 8 pound newly mention'd in the oppoſite Scale, and partly by the weight, or reſiſtance following from weight, of as much of the VVater as the Copper fills the room of; which, as experience ſhews, is one pound: And if we ſhould conceive VVater in a Veſſel adiaphorous as to Gravity and Levity to be ſubſtituted in the place of the Metalline lump, it would weigh as much as the ninth part of the Copper-lump weighed in the Air, and the ſame counterpoiſe of eight pound would maintain the *Æquilibrium*.

What the Learned Objector has at the cloſe of his Diſcourſe about the natural and artificial ballance, could not without prolixity, and

is

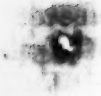
is not here neceſſary to be dwelt upon; eſpecially ſince you will ſee in what I ſuppoſe you have now received from the Preſs, in anſwer to the Ingenious Doctor *More*, what is to be ſaid on that Subject according to my Hypotheſis. VVherefore though my Learned Adverſary does in the 152. page conclude, *That Water cannot weigh in Water*, and aſſerts, that *the Preſſure of VVater* is one thing, and *VVater to Weigh in VVater* is another; yet, as I ſaid at firſt, I conceive much of our Difference may be verbal; and in my ſence, when VVater preſſes ſubjacent VVater, becauſe it does ſo upon the ſcore of its gravity, it *gravitates* in VVater, though it does not *pregravitate*, that is, actually deſcend. And ſince 'tis in the ſence of this laſt expreſſion, that our Author,

thor, if I miſtake him not, ſpeaks of weighing in VVater, his concluſion, that VVater cannot weigh in VVater, does not contradict me, who affirm not that VVater does *ſo* weigh in VVater. VVhether we ſhall agree in all other points of Hydroſtaticks, you will eaſily believe that I cannot yet tell, though by the expreſſion he is pleaſed to uſe (in the 146. page) to uſher in his Objection with, 'tis probable we may. And as to the now diſpatch'd debate, if I have imployed ſome words in another ſence than he, I preſume he is ſo equitable as to conſider, that I did not write of theſe things after having ſeen this book of his, but ſome years before, and have ſince found thoſe expreſſions juſtified by the uſe, that eminent VVriters have thought fit to make of them. And however I am glad,

that

that he has given me this opportunity of clearing my Experiment, and declaring by examples as well as words the opinion it relates to; especially, if it seems to others that I omitted to express my self so fully; my design being, as I formerly told you, to convince such Adversaries, as I then had met with, by shewing, that the above-recited *Phænomena* of the Emersion and Sinking of a Glass-Vial, depended upon the Gravity of the VVater, and not upon the positive Levity of the Air.

FINIS.



NEW
EXPERIMENTS
Of the Positive or Relative
LEVITY of BODIES
Under Water.

NEW EXPERIMENTS

Of the Poſitive or Relative Levity of Bodies under Water.

'TIs obvious, even to the Vulgar as well as to Philoſophers, that if Wood, Wax, or another body that is lighter *in ſpecie* than water, and naturally floats upon it, be detained under water, it will upon removal of that force emerge to the top. And this it does ſo readily, and, as it ſeems ſpontaneouſly, that not only the Peripatetick Schools, but the generality of Philoſophers both ancient and modern, do as well as the Vulgar

aſcribe this aſcention of lighter bodies in water to an internal principle, which they therefore call *Poſitive Levity.*

But this Principle was not always ſo univerſally receiv'd among Philoſophers, as in later ages it proved to be, *Democritus* and ſeveral of the Antients both Atomiſts and others, admitting no abſolute but only a relative or reſpective Levity, which opinion ſome of the Moderns have ingeniouſly attempted to revive.

But becauſe whatever wit they may have imploy'd in arguing; yet the Schools ſeem to have the advantage in point of Experience, the obvious inſtances, given by the Peripateticks, having neither been ſolv'd by real and practical variations of the ſame inſtances, nor counterballanc'd by new Experiments of a contrary tendency; the importance and difficulty of

of the ſubject invited me to attempt, when I was upon Hydroſtatical tryals, whether I could experimentally ſhew, that whatever becomes of the general Queſtion about Poſitive Levity, we need not admit it for the true and adæquate cauſe of the emerſion of Wood and ſuch lighter bodies, let go under water.

EXPER. I.

THe inſtance that is wont to be urg'd to prove the Poſitive Levity of Wood in Water, ſeems to me to have been too perfunctorily made to be ſafely acquieſced in. For even as it is propos'd with advantage by a learned forreign Mathematician, I cannot think it accurate enough to determine the preſent Controverſie:

for I will readily allow him to ſuppoſe, that in caſe a flat board, as for inſtance a Trencher, have its broad ſurface kept by a mans hand or other competent force upon the Horizontal bottom of a Tub full of water, if the hand or other body that detain'd it be remov'd, it will ordinarily happen that the Trencher will haſtily aſcend to the ſurface of the water. But I do not perceive, that a deciſive Experiment of this kind is eaſie (not to ſay, poſſible) to be made with ſuch materials. For the wood, whereof both the Trencher and the bottom of the Barrel conſiſt, are ſuppos'd to be lighter *in ſpecie* than Water; and to be ſo, they muſt be of a porous and not very cloſe texture. To which agrees very well, that the ſolider woods, as *Lignum Vitæ*, Braſil, &c. whoſe texture is more cloſe and compact, will not float on water but ſink in it:

And

And therefore, if there be not much more care us'd, than I have yet heard that any Experimenter has imploy'd, to bring the ſurfaces of the Trencher and the bottom of the Barrel to a true flatneſs and as much ſmoothneſs as they can be brought to, I ſhall not think the tryal ſo accurately made as it might be; not to ſay, which I ſuſpect, that though it be mentally, yet it is ſcarce practically poſſible to bring ſuch porous bodies as thoſe of the lighter woods to be fit for ſuch a contact as might be neceſſary to make the tryal accurately. And in caſe that were actually done, I ſhould be kept from expecting with my adverſaries the emerſion of the Trencher, by the Experiment by and by to be recited, and by the true reaſon of it.

I think then that the cauſe, why in ordinary inſtances, Wood, Wax, and other

other bodies ſpecifically lighter than water, being let go at the bottom of a veſſel full of that liquor, emerge to the top, is chiefly, that there is no ſuch exquiſite congruity and contact between the lowermoſt ſuperficies of the Wood, and the upper ſurface of the bottom of the Veſſel, but that the lateral parts of the Water, being impell'd by the weight of the parts of the ſame liquor incumbent on them, are made to inſinuate and get between the lower parts of the Wood and the bottom of the Veſſel, and ſo lift or thruſt upwards the Wood, which bulk for bulk is leſs heavy than the Water that extrudes it.

That this is the reaſon of the Emerſion or aſcenſion of bodies, lighter *in ſpecie* than the fluids they ſwim in, is moſt conſonant to the Laws of * Hydroſtaticks, as I have elſewhere ſhown. But whereas

* See the Hydroſtatical Paradoxes.

whereas the whole force of the argument of thoſe I diſpute with, conſiſts in a ſuppoſition, that, becauſe the Trencher (formerly ſpoken of) is plac'd upon the bottom of the Barrel, no water can come between to buoy it up, whence they conclude it muſt aſcend by an internal and poſitive principle of Levity, I thought fit to make the Experiment after another, and, if I miſtake not, a better manner.

We took then two round plates of Black Marble ſhap'd like Cheeſes, which had thoſe ſuperficies, that were to be clap'd together, ground very flat and poliſh'd very carefully, that the ſtones being laid one upon the other might touch in as many of the ſuperficial parts, as the workman could bring them to do; that, whilſt they were in that poſition, the uppermoſt being taken up, the other would ſtick

ſtick to it, and aſcend with it. And to keep out the water the better, the internal ſurfaces were, before they were put together, lightly, and but very lightly, oyl'd, which did not hinder them from moſt eaſily ſliding along one another, either forward or backwards, or to the right, or to the left, as long as the contiguous ſurfaces were kept Horizontal.

Theſe things being done, a blown Bladder of a moderate ſize was faſtned to the upper marble, and both of them were let down to the bottom of a tub of water, where, by the help of an eaſie contrivance, the lower marble was kept level to the Horizon. And now the Patrons of Poſitive Levity would have concluded, that the bladder, being a body, granted to be by vaſt odds lighter than wood, and being in an unnatural place beneath the ſurface of the water, ſhould of its own

own accord and with impetuosity emerge; but I expected a contrary event, because the bladder being tied to the upper marble, so that both of them might in our case be considered as one body, the water could not impel them up, in regard that the close contact of the surfaces of the two marbles kept the water from being able to insinuate it self between them, and consequently from getting underneath the upper marble, and pressing against the lower superficies of it. And to shew that this was the reason of the bladders not emerging, I caus'd one of the by-standers to thrust his arm down to the bottom of the tub, and with his hand to make part of the oyl'd surface of the upper marble slide off, on any side, from that of the lower, which, by reason of the smoothness and slipperiness of the surfaces, he found most easie to do. But the contact

contact ſtill continuing according to a greater part of the ſurfaces than was requiſite, I bid him yet ſlide, but by ſlow degrees, more and more of the upper marbles from the lower, till at length, when, according to his gueſs, the marbles touch'd but in one half of their ſurfaces, the endeavour of the water to extrude the bladder full of Air being ſtronger than the reſiſtance, which the contact but of part of the ſurfaces of the ſtones was able to make, they were ſuddenly diſ-joyn'd, and the bladder was by the extruding water impetuouſly, as it were, ſhot up, not only to the top of the water, but a good way beyond it.

With theſe Marbles we made ſeveral other Experiments of this kind, moſt commonly letting down the Marbles both together; but once or twice at leaſt placing the upper Marble under water upon the lower-

moſt

moſt already fixed to the bottom of the barrel.

That 'twas not the weight of the upper Marble, nor want of Lightneſs, whether poſitive or relative, of the Air included in the bladder, that kept it from aſcending, was plain, not only by the newly mention'd impetuous emerſion of it, upon the diſ-joyning of the Marbles, but by this, that the Bladder would lift up from the lower parts of the water, not only the upper ſtone when it touch'd not the other, but a weight of ſeven or eight pound hanging at it.

And that a *Fuga Vacui* was not an adequate cauſe of the coheſion of the Marbles in our Experiment, may be argued from this, that whether or no nature do any thing at any time out of abhorrence of a *Vacuum* (which may be much diſputed;) yet in our caſe this abhorrency could not be well pleaded

pleaded by its Assertors, since many of them hold it to be unlimited, and the more modest, to be at least capable of lifting up prodigious weights; whereas in our Experiment the Levity of a Bladder, that could not raise ten pound weight, was sufficient to disjoyn the marbles when they yet touch'd one another according to half their surfaces.

EXPER. II.

TO shew now whether it is not rather the Gravity and Pressure of the Water, or other ambient fluid, than the Positive Levity of a body lighter *in specie* than it, that makes the immers'd body ascend to the surface of the liquor, I devis'd this Experiment:

We

We took a bladder out of which a great part of the included Air had been expreſs'd, and tying the neck of it very cloſe, that none of the remaining Air might get out, we faſtned to it a conſiderable weight of ſome very ponderous body, as Lead or Iron. By the help of this we ſunk the bladder to the bottom of a wide mouth'd glaſs, full of water, that the ſurface of the liquor might be a good deal higher than the upper part of the bladder: This wide mouth'd glaſs we included in a great Receiver (whoſe orifice muſt be very large to be able to admit ſuch a veſſel;) which I caus'd to be carefully cemented on to the Engine. The main ſcope of this Experiment was to ſhew, that though the Air, included in the bladder, was very far from being able by its abſolute levity to lift up ſo great a weight as the bladder was clog'd with, yet the ſame

Air, continually included in the bladder, would, by its meer expanſion, without any new external heat, acquire a power of aſcending in ſpite of that weight; which aſcenſion therefore muſt be attributed to the water, which according to the Laws Hydroſtatical ought (*cæterisparibus*) to reſiſt or buoy up more potently thoſe immerſed bodies, that being lighter *in ſpecie*, than it, poſſeſs the greateſt place in it, and hinder the more water from acquiring its due ſituation: as we ſee, that among hollow ſpheres of glaſs and metal, equally thick and well ſtopp'd, there is a much heavier weight requiſite to ſink a large one than a ſmall one. For the proſecution of this tryal we began to pump the Air out of the great Receiver; and its preſſure upon the ſurface of the water being thereby more and more leſſen'd, (according to what we elſe-

where

where more fully declare,) the ſpring of the included Air began by degrees to diſtend the ſides of the bladder, till at length that veſſel of Air ſwelling every way took up ſo much more room in the water than it did before, that the water was able to lift the bladder and the annexed weight to the top, and detain it there, till we thought fit to let in again ſome of the excluded Air, which forcing that in the bladder to ſhrink in its dimenſions, the weight was preſently able to ſink it to the bottom.

And here it may be noted, that if, inſtead of hanging ſo great a weight at the neck of the bladder, we faſtned but a moderately heavy piece of Lead, ſuch as would only ſerve to ſink the bladder, and keep it at the bottom of the water, ſo that the aggregate of the Bladder, Air, and Metal, was but a little heavier than a bulk of

water equal to them: Then upon the firſt ſuck or operation of the Pump, which could withdraw but a ſmall part of the Air in the Receiver, the Air in the bladder ſuddenly expanding it ſelf, would forthwith be impetuouſly extruded by the water, though after ſome reciprocations it would float in its due poſition, till upon the return of a little outward air (ſometimes as little as we could conveniently let in) it would immediately ſubſide.

But this is not ſo neceſſary to be inſiſted on, as 'tis to take notice, that I foreſaw it may be objected, that the aſcenſion of the weight was not effected by the preſſure of the water, but by this, that Rarity and Levity being Qualities exceedingly of kinn, the great Rarefaction of the Air might proportionably increaſe the Levity of it, and conſequently enable it to perform

form much greater things than it could do before.

I will not here diſpute, whether, generally ſpeaking, a body rarified without heat, would, in *Vacuo*, or in a fluid not heavier *in ſpecie* than the body when rarified, meerly by ſuch a greater diſtance of its parts as may ſuffice to entitle it to rarefaction, become really heavier or lighter than before. I will not (I ſay) diſcuſs this queſtion here, where it may ſerve my turn to ſatisfie the recited objection by the following Experiment.

EXPER. III.

ABout the neck of a conveniently ſhap'd Viol capable to hold ſome few ounces of water, I caus'd to be carefully tied the neck of a ſmall Bladder, whence the Air had been diligently expreſs'd, ſo that the Blad-

der, being very limber of it ſelf, and probably made more ſo, as well as more impervious to Air and Water, by the fine Oyl we had caus'd it to be rubb'd with, lay upon the orifice of the Viol like a skin clapp'd together with many folds and wrinkles.

This done, we let down the Viol into a conveniently ſhap'd Veſſel full of water, and the Viol, being poyſed before-hand for that purpoſe, ſunk perpendicularly in the liquor, till the neck of the Glaſs was partly above and partly beneath the ſurface of the water: Then covering the external Glaſs with a large Receiver, we caus'd the Air to be pump'd out, and as the preſſure of that was gradually with-drawn, the Air in the floating Viol did little by little expand it ſelf into the Bladder, and unfolded the winkles of it, till at length it became full blown without altering the erected

ſted poſture of the Glaſs it lean'd upon. But this great expanſion, being made above the Water, and conſequently in a medium not heavier than the included Air, gave that highly rarified Air no ſuch increaſe of Levity, as enabled us to perceive, that it made ſo much as the neck of the Glaſs ariſe higher in the Water than it did before. Nor did we take notice, that the return of the Air into the Receiver, by reducing the Air in the Bladder to its former unrarify'd eſtate, made the Glaſs ſink deeper than before. But when the Experiment was tryed with the ſame Glaſs and Bladder at the bottom of the Water, then, upon the pumping out the Air, the Bladder being dilated under water was after a while carried up to the top, and took up with it about eight or ten ounces, that had been, to clog it, faſten'd to the bottom of the Viol.

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A

NEW
EXPERIMENTS
About the PRESSURE of the
AIR'S SPRING
On Bodies under Water.

NEW EXPERIMENTS

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AIRS SPRING

On Bodies under Water.

NEW EXPERIMENTS

About the Preſſure of the Air's Spring on Bodies under Water.

I Do not think it were difficult for an intelligent peruſer of our Phyſico-Mechanical Experiments, to find there divers *Phænomena*, whence it may be deduc'd, that Bodies under water, though kept by that liquor from the immediate contact of the Air, may yet be expos'd to its preſſure (whether the Air act as having a Weight or as a Spring.) But becauſe not only the Vulgar, but Philoſophers have been ſo long and generally poſſeſs'd with an opinion, that a fluid ſo little heavy as

as the Air, cannot by its weight act upon a liquor, that is, like water, bulk for bulk a thouſand times heavier than it: And becauſe alſo it ſeems yet more ſtrange, that a little Air, perhaps not amounting to a ſcruple or drachm in weight, ſhould in its ordinary ſtate of Laxity act conſiderably upon Bodies, which, being cover'd with water, ſeem by the interpoſition of that liquor to be fenced from the incumbent Air; it may be worth while to add three or four Hydroſtatical Experiments, to confirm a Truth that very few are yet acquainted with; and add to the proofs, already given of the power of the Spring of the Air, ſome of the operations we have diſcovered it to have upon Bodies plac'd under water.

There are two ſorts of Tryals, that I ſhall imploy to ſhew, that a ſmall quantity of incloſed Air may by its preſſure

preſſure (which in our caſes muſt depend upon its Spring) have a conſiderable operation upon bodies under water, notwithſtanding the interpoſition of that liquor.

For, this preſſure we ſpeak of, may be manifeſted, in the firſt place by what it directly and poſitively operates upon bodies covered with water: And in the next place, by the things that regularly enſue upon the removal of the inclos'd Air, or the weakning of its Spring.

EXPER. I.

TO begin with the former way of ſhewing the preſſure of the Air, I thought it ſufficient, in regard of the Tryals to be referr'd to the ſecond way, to make the following Experiment.

We

We took a ſquare Glaſs-Viol, gueſs'd to be capable of holding between half a pint and a pint of water; the neck of this we luted on carefully and ſtrongly (for elſe it would have been buoy'd up) over the orifice of the ſmall pipe, at which the Air paſſes in our Engine out of the Receiver into the Pump: Then whelming over this glaſs a great Receiver, we luted it ſtrongly to the Engine (that it might as well keep in the Water as keep out the Air) and at the top poured in as much water as ſufficed to inviron the internal Receiver (if I may ſo call it) and cover it to a pretty height. This done, we exactly cloſed with a turning key the hole in the great Receiver, at which the Water had been poured in, that no air might get in or out that way. And laſtly we began to pump out the Air contain'd in the internal Receiver;

to the end that that Air, which by the above-mention'd pipe had Communication with the External Air, might no longer by its preſſure aſſiſt the glaſs to reſiſt the preſſure, which the incumbent and inclos'd Air, by vertue of its Spring, conſtantly exerciſes upon the ſubjacent water, and by its intervention upon the ſides and bottom of the internal Receiver.

And as we expected not, that this glaſs by its own ſingle force ſhould reſiſt the preſſure of the Air inclos'd in the upper part of the great Receiver notwithſtanding the interpoſition of the water; ſo the event fully juſtified our conjecture: For at the firſt exuction, which could not be ſuppos'd to have well emptied the internal glaſs, this veſſel was, by the preſſure of the ſuperior Air upon the circumſtant water, broken into I know not how many pieces. And the ſame

Experi-

Experiment, though with a little flower ſucceſs, was repeated with a ſtronger internal glaſs.

EXPER. II.

I Proceed now to the ſecond way of manifeſting the preſſure of inclos'd Air upon Bodies under water, which is by ſhewing the *Phænomena*, exhibited by thoſe Bodies upon the removal or leſſening of that preſſure.

Having ſqueez'd out of a moderately ſized Bladder the greateſt part of its Air, we tied the neck of it very cloſe, and then faſtning to it a competent weight, we plac'd it at the bottom of the talleſt and largeſt glaſs we could cover with our great Receiver, that ſo, though the incumbent Air

were

were pump'd out, none of the Water might be pump'd out with it, but ſtill retain the ſame height above the Bladder. Having then poured upon the Bladder as much Water as would ſwim a great way above the upper part of it, we cover'd this glaſs of Water with a great Receiver, which being carefully cemented on to the Engine, the Pump was ſet a-work, and as the Air, which by its Spring preſs'd upon the ſurface of the included Water, was by degrees pump'd out, ſo the Air that was impriſon'd in the Bladder, did gradually expand it ſelf at the bottom of the Water, as if no ſuch liquor had interpos'd between them otherwiſe than by its weight, upon whoſe account it muſt be allowed to give ſome little impediment to the expanſion of the Bladder in proportion to the height it had above it.

The Event of our Experiment was such as was expected, namely that the immers'd Bladder was at length full blown, by the dilatation of the Air, inclos'd in it; and by its intumescence made a considerable part of the Water run over by the sides of the glass, that before contained it all. And when access was given again to the external Air, the internal being compressed, the Bladder was presently reduc'd to its wrinkled state.

EXPER. III.

WE took a small but fine Bladder, whose neck was strongly tied up, when it was, by guess, about half full of Air: This we put into a short brass Cylinder, the lower of whose bases was clos'd with a Brass-

Braſs-plate, and the other left open; this open orifice we afterwards ſtop'd, but not exactly, with a Cylindrical plugg, that was ſomewhat leſs wide than it, and was by a rim at the top hinder'd from reaching too deep into the cavity of the Cylinder, that it might not do miſchief to the Bladder that lay there beneath it; upon this plugg we plac'd an almoſt Conically ſhap'd weight of Lead, and this pile of ſeveral things being ſo plac'd upon our Engine, that we could cover it with a great Receiver, we carefully cemented on this veſſel, and at the top of it poured in ſo much water as would ſerve to fill the vacant part of the braſs Cylinder, and the cavity of the Engine to ſuch a height, that it cover'd all the leaden weight, which was ſeveral inches high, except a rim which was faſtned to the top of it for the convenienter removing of it.

All this being done the Pump was ſet a-work, and long before we had exhauſted the Air of the Receiver, that which was inclos'd in the lank bladder had by degrees diſplay'd ſo vigorous a ſpring, that it had heav'd up the weight that lay upon it to a notable height, and kept it there till the Air was let in from without to aſſiſt its being depreſs'd by the leaden weight, which amounted to no leſs than about 28. pound.

EXPER. IV.

THere remain'd yet one tryal to be made, which in caſe it ſhould ſucceed, ſeem'd likely to appear as great an evidence of the force of the Air's Spring upon bodies under water, as could be reaſonably deſired of

of us; it having been look'd upon by many *Virtuoſi* as the conſiderableſt inſtance of the force of the Air's Spring even when no water interven'd in the tryal.

To ſatisfie therefore our curioſity, we took a copper Veſſel of a Cylindrical ſhape, and a conſiderable height; into this, being firſt almoſt filled with water, we put a ſquare Glaſs-Vial capable by gueſs to hold nine or ten ounces of water, and exactly ſtop'd with a cork and a cloſe Cement; this Vial by a competent weight was detain'd at the bottom of the water, from whoſe upper ſurface it was conſiderably diſtant: then the Copper Veſſel being plac'd upon the Engine, and included in a great Receiver well cemented on, the Air was by degrees pump'd out, but before it was quite exhauſted, the Glaſs at the bottom of the water was, by the

ſpring of the Air included in it, burſt into many pieces, not without great noiſe, and a kind of ſmoak or miſt that appear'd above the ſurface of the water.

Another Glaſs of the ſame ſort had been broken after the ſame manner in another Veſſel; but having afforded us no particular *Phænomenon*, I barely mention it, to ſhew that we made more than one tryal of this kind.

The conſequence that will naturally reſult from the three laſt Experiments, is this, that ſince barely upon the withdrawing of the preſſure of the included Air (which was perhaps but very little in quantity,) the Air reſiding in the immers'd bodies, did, by vertue of its Spring, expand it ſelf ſo forcibly as we have recited, and perform notable things, the Air above the Water muſt have exercis'd

a

a very powerful preſſure upon the ſurface of it, ſince, (ſetting aſide the weight of the water, of ſmall moment in our tryals,) it muſt have been at leaſt æquivalent to (and probably much exceeded) that force of the immers'd Air, whoſe exerciſe it was able totally to hinder.

And from hence it may be eaſily deduc'd, that the weight of the Atmoſphere acts upon bodies under water, notwithſtanding that the interpos'd liquor is by vaſt odds heavier *in ſpecie* than Air; for, we have juſt now prov'd the preſſure of inclos'd Air, (which conſiſts in its Spring,) upon bodies under water; and 'tis manifeſt, that the ſtrength of the Spring of this inferiour Air, we make our tryals with, is caus'd by the weight of the ſuperiour Air, which bends and compreſſes thoſe little Aereal ſpringy particles, whereof our

Air consists; so that the weight of the Atmosphere being æquivalent to the Spring of the inferiour Air, (for else it could not compress it as much as it does,) must lean upon the surface of the subjacent water, with a force æquivalent to the spring of that part of it that is contiguous to the water.

This Experiment brings into my mind another that I once made, which though not properly Hydrostatical, yet relating to positive Levity, may perhaps be not uselesly added on this occasion; wherefore I shall here subjoyn a transcript of the *Phænomenon*, that belongs to our present purpose, as 'tis registred soon after the Experiment was made.

[To examine by a visible Experiment the common doctrine, that a portion of Air, by being much dilated, rarified or expanded, does acquire

acquire a new and proportionable degree of Positive Levity, I devis'd to put in practice the following way:

We took a Bladder of a moderate size, that was very fine and limber, that it might be the lighter and more easily distended. The most part of the Air being squeez'd out of the Bladder, the neck of it was tied up very close, that no air might get out of it, nor any (external) get into it. This limber Bladder was hung at one of the Scales of a Ballance, whose Beam had been purposely made more than ordinarily short, that the instrument, (which yet was tickliſh enough) might be suspended, and capable of playing in the cavity of a great Receiver, into which we conveyed it, having first carefully counterpoyſed the Bladder with a metalline weight put into the opposite scale.

This done, the Air was pump'd out,

out, and as that was withdrawn, the Bladder was more and more expanded by the Spring of the internal Air, till at length, when the Receiver was well exhauſted, it appear'd to be quite full. Notwithſtanding which great dilatation of the included Air, it did not appear by the depreſſion of the oppoſite ſcale, to be grown manifeſtly lighter than it was at firſt. And the Bladder ſeem'd alſo to retain the ſame weight, after it had, by the Air that was let into the Receiver, been compreſſed into its former wrinkled ſtate.]

NEW

NEW
EXPERIMENTS
About the Differing
PRESSURE
Of Heavy
SOLIDS and FLUIDS.

NEW EXPERIMENTS

About the differing Preſſure of Heavy Solids and Fluids.

SInce not only in vulgar Spectators of Phyſico-Mechanical Experiments, but even among ſome Learned men it has prov'd a great impediment to mens freely acquieſcing in the doctrine founded on thoſe *Phænomena*, that if the Atmoſphere could really exerciſe ſo great a preſſure as we aſcribe to it, it would unavoidably oppreſs and cruſh all the bodies expos'd to it, and conſequently neither other Animals, nor Men would be able to move under ſo great a load, or ſubſiſt in ſpite of ſo forcible a compreſſion.

This

This I readily grant to be a plausible Objection, but I suppose the force of it will be taken away by the following considerations put together.

And first, the power of pressing, that we ascribe to the Air, is not a thing deduced, as too many other consequences in Physick are, from doubtful suppositions or bare *Hypotheses*, but from real and sensible Experiments. And therefore since we have clear and positive proofs of the Pressure of the Air, though we could not explain how Men and other Animals are not destroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifests to be a Truth: As is generally practised in treating of the Attractive and other powers of the Loadstone, which are freely acknowledged even by those that confess

fess themselves unable to explicate them; though, if experience did not satisfie us of them, they were liable to divers more considerable objections, than any that is urg'd against the Pressure of the Air.

Secondly, but though it be not absolutely necessary that we should answer the above-recited Objection otherwise, than by thus declaring that the Spring of the Air is not to be rejected for it; yet we will endeavour very much to lessen it, if not quite remove the difficulty, before we put an end to the discourse.

I consider then thirdly, that they that urge the lately mention'd Objection against the great Pressure of the Air, seem not to be aware, that we were conceived and born in places exposed to the pressure of the Atmosphere, and therefore how great soever that pressure appear to be, it ought

ought not to crush us now, since when we were but embryos or new-born Babes, and consequently very much more weak and tender than we now are, we were able to resist it, and not only live, but grow in all dimensions in spite of it.

If there were any place about the Moon, or some other of the Celestial Globes, that some Learned men fancy to be inhabited, that has no Atmosphere, or equivalent Fluid about it, and where yet men could be generated a-new, if one of those men should be supposed to be transported thence, and set down upon our Earth, there might be made an Experiment fitted for our controversie. In the mean time I doubt, that since Nature is not observed to make things superfluously strong, such a humane body being not made to resist any weight or pressure of Air, would be of so tender

and

and compreſſible a make, that it would eaſily be cruſhed inwards by our Atmoſpherical preſſure. And though we cannot give an inſtance of this kind, yet we make tryals ſomewhat Analogous to it in our Pneumatical Engine. For when we place water in our Receiver, and pump out the Air that was above it, there will be generated a multitude of bubles, ſome of which, when the Air is carefully withdrawn, will be of a ſtrange and ſcarce credible bigneſs; theſe bubles being generated where the Air cannot preſs upon them, theſe dimenſions are ſo natural to them, that if the Receiver be ſuppos'd not to leak, nor other unfriendly accidents to intervene, they would (for ought we know) laſt a good while; ſince I have elſewhere ſhewn, that the Spring of highly dilated Air did continue for many months, and a bladder

would for no leſs time continue blown and filled in our *Vacuum* by a little Air that was left in it, when the ambient air began to be withdrawn from it. And yet the large bubles above mentioned, when once the outward air is ſuffered to come in upon them, are thereby ſo violently compreſs'd, that in a trice they ſhrink into dimenſions, too ſmall to keep them ſo much as viſible; and if I could have ſucceeded in my Attempt of producing ſuch living Bodies as I endeavour'd (but did not expect) in our *Vacuum*, I ſuppoſe the ſucceſs would have confirmed what I have been ſaying.

Fourthly; but you will tell me, that ſo great a weight and preſſure, as I aſſign the Atmoſphere, muſt needs make a man feel pain, and, if not otherwiſe diſlocate ſome of the parts, muſt at leaſt preſs the whole body inward.

But

But first, being accustom'd to the pressure from our very birth, and even before it, so early and long an accustomance hinders us from taking notice of it; those pressures only being sensible to us, that are made so by some additional cause, which by making a new impression excites us to take notice of it. So we are not sensible of the weight of the cloths we are accustom'd to wear; and so a healthy man is not sensible of the heat in his heart because 'tis constant there, and the sentient parts of the heart have been still us'd to it, whereas that heat often-times has been very considerable; and when in living dissections a man puts his finger into the heart of an Animal, which probably has a fainter, or at least no stronger degree of heat than a humane heart, he will feel in his fingers, accustom'd to the Air, a manifest de-

gree of heat, if they be but in their uſual temper. 2. I have elſewhere proved by Experiments, that a cubick inch of Air, for inſtance, has as ſtrong a ſpring as ſuffices to enable it to reſiſt the weight of the whole Atmoſphere, as far as it is expoſed thereunto; for elſe it would be more compreſs'd than *de facto* it is. And 3. I have alſo ſhewn, that a very little portion of Air, though it will much ſooner looſe its ſpring by expanſion than a greater, yet 'twill reſiſt further compreſſion as much as a greater. And 4. I have alſo ſhewn, that in the pores of the parts of Animals, whether fluid or conſiſtent, as in their Blood, Galls, Urines, Hearts, Livers, *&c.* there are included a multitude of Aereal corpuſcles, as may appear by the numerous bubles afforded by ſuch Liquors, and the ſwelling or expanſion of the conſiſtent parts in our exhauſted

hausted Receiver. 5. To this we may add, that, besides the Bones, whose solidity is not questioned, a much greater part of the humane body than is wont to be imagined, does really consist of Membranes and Fibers, and the coalitions and contextures of these; and that these substances are by the Providence of the most wise Author of Things made of a much closer and stronger Texture, than those, that have not tried, will be apt to think; as I could make probable by the great force that Bladders will endure, and the very great weight that Tendons of no great thickness will lift up or sustain, and by other things that I shall not now insist on. Lastly, There is a far greater difference, than men are wont to suspect, between the effects of the Pressures made upon Bodies by incumbent or otherwise applied solid

weights, and those that they suffer from heavy but every way ambient fluids; as will appear by the Experiments to be mention'd by and by.

From the particulars contained in these considerations, we may be assisted to shew, why 'tis not necessary, that the pressure of the Atmosphere, though as great as we suppose it, should oppress and crush the bodies of men that live under it: for, the solidity of the bones and the strong Texture of the membranes and fibres, and the spring of the Aereal particles, that abound in the softer as well as in the fluid parts of bodies, is equivalent to the pressure of as much of the Atmosphere, as can exercise its pressure against them, and makes the frame of a humane body so firm, that it may well resist the pressure of the outward Air, without having

ving any part violently diſlocated, whilſt the external preſſure is exerciſed but by the Air, which being but an invironing fluid, preſſes it equally (as to ſenſe) on every ſide. And becauſe our bodies have been produced in the Atmoſphere, and from our very birth expoſed, without intermiſſion, to the preſſure of it; our continual accuſtomance to this preſſure, and the firmneſs of their ſtructure, keep us from being ſenſible of the weight or preſſure. And that it was not impertinent for me to mention the firmneſs of the frame of our bodies on this occaſion, I ſhall manifeſt by an inſtance, that will upon another account alſo be proper for this place.

We know, that multitudes of men have had occaſion to paſs over high mountains; and, beſides that I have been my ſelf upon the Alps and

Apennines, I have enquired of Travellers, that have visited the Asian and American Mountains, and some that have been upon the top of the Pick of *Tenerif* it self: But though divers of them took notice of a great difference in the Air at the top and bottom as to some other Quality, as coldness and thinness; yet I never met with; nor heard of any, that took notice of a difference as to the Weight of Air he sustained, or that complained, that when he was come down to the foot of the Mountain, he felt any greater compression from the Air than at the top. And yet the Experiments made as well by others as by our selves, sufficiently witness, than on more elevated parts of the Earth, which have a less height of the Atmosphere incumbent on them, the weight and pressure of the Air is not so great as below. And on very high Mountains

tains, 'tis not unlikely that this difference may be very considerable, since, when the *Torricellian* Experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, (which is none of the highest mountains in the world, being found by the ingenious makers of that observation to be but about 500 Fathoms,) they found the difference of the Mercury at the top and bottom to amount to about three inches: And consequently, if the tryal had been made with Water instead of Quicksilver, the difference would have been about three foot and a half in the perpendicular height of the Water. And 'tis very probable, that in much higher Mountains, the difference of the Mercurial Cylinders height at the top and bottom may be much greater; and at the bottom of some very deep Well or Mineral groove, which may without improbability

bility be ſuppoſed to be placed at or near the foot of one of theſe Mountains, if we conceive the Baroſcope to be let down, the variation of the height of the Mercurial Cylinder may be yet much more conſiderable; and yet we find not that the diggers in the deepeſt Mines, in mountainous Countries, are ſenſible of being lean'd on or compreſs'd by any unuſual weight. But not here to build on any thing but matter of fact, it appears by the newly named obſervation, that, when a man was at the bottom of the hill, he had as much greater weight of Air leaning upon his head than he had at the top, as was equal to the height of an imaginary veſſel full of water, which having his head for baſis, were three foot and a half high: which is ſo conſiderable a weight, as could not but have been, not only ſenſible, but very troubleſome

ome and uneasie to support. And what has been said of the gravity of a pail of water, that leaned on his head, may be proportionably appli'd to his Shoulders, Arms, *&c.*

Whence I think I may infer, that the reason, why such a weight was not felt by the man it compress'd, was not, that the Air, that pressed him, was not considerable, but that the pressure was exercised after the uniform manner of fluid Bodies.

And this may suffice to shew that there is no necessity that the compression of the Atmosphere should make it impossible to live in it. But because 'tis observ'd, that those that dive to great depths under water, are not oppressed by the great weight of the incumbent water, and the cause of this strange *Phænomenon* is not so easie to be assigned, and therefore has been made one of the two grand arguments

ments, whereon the non-gravitation of water in water, and air in air has been, and still remains, founded: I shall here offer something *ex abundanti* towards the solution of that noble and difficult Problem.

And first, that what is observ'd by the Divers, does not evince that water does not weigh in water, I have elsewhere * prov'd by such reasons and Experiments as had the good fortune to convince eminently learned men, that were sufficiently prepossess'd with the vulgar opinion: And in the same Treatise I have given a clear account, why a Bucket full of water is not felt considerably heavy, whilst 'tis under water, in comparison of what 'tis whilst 'tis drawn up into the Air; which is the other *Phænomenon* that I freshly intimated the common Opinion to be founded on.

* See the Hydrostatical Paradoxes.

Next,

Next, I do not think it ſtrange, that that follows not which 'tis objected ſhould follow from our *Hypotheſis*; namely, that a Diver ſhould be violently depreſs'd to the bottom of the water, by the weight of ſo great a Pillar of the Sea as is plac'd perpendicularly over his body. For if we imagine a plane ſo to cut the Sea-water, as to paſs by the Divers body; then as that part of the plane, on which his body leans, will be preſs'd by It, together with the water that is perpendicularly incumbent on it; ſo all the other parts of the ſame plane will be preſſed by equally tall Pillars of water perpendicularly incumbent on them; and conſequently, if the mans body were juſt equiponderant to an equal bulk of water, it and the water that leans on it would be ſuſtain'd by the preſſure of the collateral water incumbent on the other parts of the

ſame

same plane (as may be easily understood by what I have elsewhere said.*)

* See Appendices to the Hydrost. Paradox.

And therefore there is no reason, why the Divers bodies should be more forcibly depress'd than its depression is resisted. 'Tis true, that this body will sink, but that is because 'tis not only, as we lately suppos'd it, æquiponderant to an equal bulk of water, but heavier than that. But then, since the Water by its gravity and resistance takes off as much of the weight of the Divers body, whilst that is immers'd, as a quantity of water equal to it would weigh in the Air, the subsiding of the humane body by its own weight ought to be but slow, because that being not *in specie* much heavier than water, it can sink but by virtue of the surplusage of weight that it has above water. And in effect, I have been informed by Swimmers, that in the Sea, whose

whose water by reason of the Saltness is specifically heavier than the common water, they could hardly dive when they had a mind; the salt-water did so much support them. And having, because I had no conveniencies to make tryals upon the parts of humane bodies, examin'd the weight of parts of other Animals in Air and Water, I found the overplus of the weight of the animal substances above an equal bulk of water to be but very small. And this may suffice to take off the wonder, why, though water be admitted to gravitate in water, yet Divers are not depress'd by that which leans upon them; the endeavour, they use to keep themselves from sinking by striking the resisting water with their arms and legs, easily compensating their weak tendency downwards, which the small surplusage of gravity above-mentioned gives them.

But

But it seems to me far more difficult to render a reason, why those that are a hundred foot beneath the surface of the Sea, are not crush'd inwards, especially in their chests and abdomens, or at least so compress'd as to endure a very great pain.

To clear up or lessen this difficulty, I have two things to offer.

1. I confess, that I am not intirely satisfied about the matter of fact; for I do not yet know, whether it fares alike with the Divers at all depths under water: for, according to the answers I obtain'd from persons that had been one of them at the Coral-fishing in the *Streights*, and the other at the Pearl-fishing near *Manar*, I do not find that the Divers are wont to descend to the greatest depths of the Sea, which if they did perhaps they would find a notable difference.

And in small, or but moderate depths

depths, those that dive without Engines usually make such haste, or are so confounded, or have their minds so intent upon their work, that they take not notice of such lesser alterations, as else they might observe, especially they being persons void of curiosity and skill to make such observations. Which I the rather mention, because having met with a Learned Physician, that living by the Sea-side in a hot climate, delighted himself much in diving; and inquiring of him whether he felt no compression, when he passed out of the Air into the Water, he answered me, that when he div'd nimbly as others use to do, he took not notice of it, but when he let himself sink leisurely into the water, he was sensible of an unusual pressure against his *thorax*, which he several times observed.

A man that gets his living by

fetching up goods out of wrack'd Ships, complain'd to me, that if with his diving Bell he went very deep into the Sea, and made some stay there, he found himself much incommodated; which though he imputed to the coldness of the water, yet by the symptoms he related I was inclin'd to suspect, that the pressure of it upon the *Genus Nervosum* might have an interest in the troublesome effect. And I have been assured by an eminent *Virtuoso* of my acquaintance, that he was lately informed by a person, whose profession it is to fetch up things from the bottom of the Sea by the help of a diving Bell, that several times when he descended to a great depth under the surface of the water, he was so compress'd by it, that the blood was squeez'd out at his Nose and Eyes; which Relation seems to favour our conjecture, and would

much more confirm it, if I were ſure, that the effect was no way caus'd by ſome fermentation or other commotion in the blood it ſelf, occaſioned by the great denſity or other alterations of the Air he breath'd in and out, or by ſome other operation of the ambient *Medium* diſtinguiſhable from the compreſſion of the water, though perhaps conjoyn'd with it.

And on this occaſion I remember, that queſtioning an Ingeneer, who had made uſe of an Engine to go under water, quite differing from the Diving-bell; he anſwer'd me, that when he came to a conſiderable depth, he found the preſſure ſo great againſt the Leathern caſe, wherein he deſcended, and by that means againſt his belly and *thorax*, that he feared it would have ſpoiled him, which forced him to make haſte up again. But this obſervation, to have much built

upon it, ſhould be further inquired into.

Theſe things, and not theſe only, make me wiſh, that what is felt by thoſe that dive to great depths, and ſtay at them, might be more heedfully obſerv'd by intelligent men, that being fully inform'd what is true in point of fact, we may the better and more chearfully indagate the reaſons.

In the mean while, taking things as they are thought to appear, I ſhall propoſe two things towards the ſolution of our difficulty; namely the Firmneſs of the ſtructure of a humane body, and the Uniformity of the preſſure made by fluids.

Of the firſt of theſe I ſhall add but little to what has been already ſaid, where I ſpoke of the reſiſtance made by our bodies to the compreſſion of the Atmoſphere; only ſhall here take notice,

notice, that whereas the Membranes are very thin parts, and therefore ſeem unfit to make any great reſiſtance; we have tried, that if a piece of fine Bladder were faſten'd to the orifice of a Braſs-pipe of about an inch in Diameter, we could not by drawing the Air from beneath it, make the weight of the Atmoſphere break the bladder, though the weight were perhaps æquivalent to an erected Cylinder of water, of the wideneſs of the orifice and about 30. foot high, and were indeed ſuch, that divers men, that laid their hands on the orifice when the Air was pump'd out from beneath, complain'd, that they were not able to lift off their hands again till ſome of the Air was readmitted.

But the main thing, I ſhall propoſe towards the ſolving of the difficulty we are conſidering, is the Uniformity wherewith fluid bodies preſs

upon the ſolid ones that are placed in them. And becauſe I remember not to have met with Experiments purpoſely made to ſhew, how this ſort of preſſure is more eaſie to be reſiſted than that of ſolids againſt ſolids, I ſhall ſubjoyn the following tryals.

EXPER. I.

EXPER. I.

IN the ſhort Cylinder of Braſs above-mentioned we put a fine Bladder tied ſo cloſe at the neck, that none of the Air (whereof it was about half full) could probably get out. Which we did, to the end that the Hen-Egge, we were to bed in it, might lie ſoft, and have its ſides almoſt cover'd with the limber and flaccid Bladder and contained Air: This done, we covered the remaining part of the Egg with another Bladder, that nothing that was hard might come to bear immediately upon the ſhell: then we put the wooden plugg into the Cylinder and a weight upon the plugg, which is to be done very ſlowly and warily, leſt the quick deſcent of the weight ſhould make the plugg break the Egg it leans on.

Laſtly, the Cylinder thus fitted, being cover'd with a large Receiver, and the Air being drawn out, that air, which was tied up in the Bladders, by degrees expanded it ſelf ſo ſtrongly, as to lift up the plugg and the incumbent weight to a pretty heigth, and keep it there till the external Air was readmitted.

Now ſince 'twill be readily granted, and appears by divers Experiments elſewhere related, that the Air in ſuch caſes expands it ſelf vigorouſly every way, it appears by the recited tryal, that it preſſed againſt the Egg with the ſame force, that it preſs'd proportionably againſt the bottom of the Plugg, and that force was more than ſufficient to lift up the weight, which (together with the Plugg) amounted to about thirty pound, and yet the Egg being taken out appeared perfectly whole and no

way

way harmed; whereas upon the ſame Egg (if I miſtake not) or at leaſt another of the ſame kind, laying warily a while after ſmall weights one upon another, the Egg was cruſh'd to pieces by about four pound weight. This Experiment, though it ſeem'd conſiderable to thoſe that ſaw it, and may prevent an objection, for which reaſon I here mention it; yet will appear in no way ſtrange to them that conſider, that the weight of the Atmoſphere, which the Egg ſupported, before it was put into the Cylinder, was more than æquivalent to ſuch a preſſure of the Air, as may ſuffice to lift up the plugg: Wherefore I thought fit to make further tryals of a differing nature.

EXP. II. We took a Glaſs-buble of about an inch and half in Diameter, which we cauſed to be blown at

at the flame of a Lamp, that it might be far more thin and eaſie to break, than the thinneſt Vials that are wont to be blown in the Glaſſer's Furnaces. This Buble we included between Bladders, as we did the Egg in the former Experiment; and then having warily put the plugg into the Cylinder, ſo as it might preſs upon the Bladder that inviron'd the Glaſs, we leiſurely put the weights upon the Plugg, till they together with the Plugg amounted to 30. pound or more, which being removed, the Plugg was taken out, and the Glaſs-buble, though it were extraordinarily thin, (perhaps no thinner than fine white Paper) was taken out whole.

EXP. III. But leſt the great reſiſtance of ſo thin a Glaſs (which yet was not Hermetically ſealed) ſhould be aſcribed to the Sphæricalneſs of its figure,

figure, we imploy'd, instead of it, the shell of an Egg, whence by a hole, made at one end of it, the Yolk and White had been taken out. This empty and imperfectly closed shell we handled as we did the Glass-buble in the former Experiment; and, notwithstanding the great leaden weight, that leaned by the intervention of the plugg upon the soft body, that invironed it, It was taken out, not only uncrushed together, but, for ought we could perceive, without the least crack.

EXP. IV. And to shew, that what we observed about the nature of the compression of fluid bodies will hold as well in Water as Air, though it seemed difficult to make the tryal with the accommodations we then had, we thought upon the following Expedient.

Into

Into a limber Bladder, almoſt full of water, we put a Hen-Egg, and tying the neck very ſtrait, that nothing might get in or out, we ſo plac'd the Bladder in the Braſs-Cylinder, that the Egg might not be immediately touched by any thing that was hard; then putting the Plugg into the Cylinder, we warily and leiſurely heaped upon it flat-bottom'd weights of Lead conveniently ſhaped, till they amounted (if both I and another miſremember not) to about 75 pound; notwithſtanding all which the Egg was taken out ſound and uncrack'd, and probably might have ſupported a much greater preſſure, if we had been furniſhed with more weights of a commodious figure to heap upon it.

If we compare with this what was noted at the cloſe of the firſt Experiment, about the breaking of an Egg with

with four pound weight, when no fluid body was interpoſed, it will be obvious to conclude, how great a difference there is between the reſiſtance that a body may make to the preſſure of ſolid bodies, that bear hard againſt ſome parts and not againſt others; and its reſiſtance to others that compreſs it uniformly or in all places alike. For though it be denied, and that, I think, upon very inſufficient grounds, that bodies under water are preſſed by the incumbent water, becauſe, as 'tis pretended, the Elements gravitate not in their proper place; yet this objection cannot be pretended to take place in our laſt Experiment, where the main thing that leaned upon the water, which ſurrounded the Egg, being not a Pillar of Homogeneous water, but a great and ſolid weight of Lead, the included Egg muſt by the intervention of

of the water have been compressed. Nor were Eggs the only bodies we endeavour'd to crush after this manner, the tryal having been also made upon a substance more soft and of a very irregular shape.

To apply this now to Divers, when they are at a moderate depth under water; it seems not improbable, that the structure of their bodies should be robust enough not to be violated by the pressure of the incumbent and otherwise ambient water. For we have seen by the former Experiments, and especially by the last recited, that a body, easie to be broken inwards by an incumbent solid weight, will remain intire and unaltered in point of figure, under a very much greater weight that compresses it after the manner of an ambient fluid. And though it would seem to many, that even in our supposition the *Thorax*, being

being, as they think it, a kind of empty ſpace in the body, the ribs and muſcles ought by the weight of the water to be cruſhed into the great cavity intercepted between them; yet it is to be conſidered on the other ſide, that the Air contained in the cheſt, eſpecially when its Spring is increaſed by thoſe accidental cauſes, that may take place when men are deep under water, particularly the præternatural heat, which the want of the uſual reſpiration is apt to produce, will very much help the cheſt to reſiſt the preſſure, as they will eaſily grant, that have tryed the reſiſtance, that Air makes, to be conſiderably compreſſed under water, the difficulty of farther compreſſing it ſtill encreaſing, as in Springs it ought to do, the more it is compreſſed. And I further obſerve, that the ſtructure of the *Thorax* is much

more

more firm than men are wont to ſuppoſe; as appears by the very great ſolid weights, that ſome men do, for gain, or to ſhew their ſtrength, ſuffer to be laid on their breaſts, without receiving any miſchief thereby. And if I ſhould admit, that at great depths, the water had ſome little compreſſive operation upon the cheſt; yet that can be no other than the preſſing the parts a little inwards, and that the ſtructure of the *Thorax* it ſelf, fitted by nature for conſtriction and dilatation (as may appear in vehement takings in and blowings out of the Air) may admit with ſmall inconvenience. To which purpoſe I recall to mind, what I lately mentioned concerning the Phyſician, that found his *Thorax* ſomewhat compreſſed when he leiſurely dived; as alſo what I have * elſewhere delivered concerning

* In the Append. to Hydroſt. Paradox.

ing a Tad-pole, which ſwimming in water that was ſtrongly compreſs'd, by an external force, ſeemed thorough the Glaſs, that contained the water, to be ſomewhat leſſen'd in bulk, and yet not killed, nor ſenſibly cruſhed notwithſtanding its great tenderneſs. And if there were parts of a human body, that were of a Texture too weak and too diſproportionate to the reſt, I think it poſſible, that this compreſſion inwards might be great enough to be very ſenſible to the Divers. For having purpoſely inquired of a certain man, whoſe trade 'twas to fetch up goods out of Ships caſt away, by the help of a diving inſtrument, he told me, that when he was at a conſiderable depth under water, as about ten or twelve Fathoms, he found, ſuitably to my conjecture, ſo great a preſſure againſt the drums or thin

membranes of his Ears, which were not ſufficiently counterpreſs'd from within, as put him to a great deal of pain, till he had found ſome contrivances to leſſen the inconvenience. Nor was this man the only Diver that has complained of this troubleſome preſſure, which ſeems to argue, that, at leaſt at great depths under water, the firmneſs of the ſtructure of a mans body does concurr with the uniformity of the fluids preſſure, to keep him from being hurt by the incumbent and otherwiſe ambient water.

But I ſhall now ſay no more of the Problem obout Divers, ſince (beſides that the matter of fact is not yet in my opinion accurately enough ſtated and determined,) the true ſolution of it is not neceſſary to give a reaſon, why the weight of the Air, a fluid ſo much lighter than

than water, ſhould not oppreſs nor cruſh the bodies of Animals; though what has been already ſaid about the reſiſtance of bodies under water, may ſerve very much to confirm the reaſons I propos'd, why we that live in the Atmoſphere are not (ſenſibly) compreſs'd, much leſs oppreſs'd by its weight.

FINIS.

ADVERTISEMENT.

THE Reader is desired to take notice, that of this Tract concerning the *Differing Pressure of heavy Solids and Fluids*, there have been lost, by the carelessness of the Printer, Eleven written pages, which he under his hand had acknowledged to have received; and with the contents of which (many of them being Quantities, and other circumstances of Experiments, formerly made,) the Author cannot now charge his memory.

Ash. C. 9.